THE PHYSICAL SOCIETY OF LONDON.

PROCEEDINGS.

VOLUME 36.—PART 3.

APRIL 15, 1924.

Price to Non-Fellows, **6s.** *net, post free* **6/3.**

Annual Subscription, **30/-** *post free, payable in advance.*

Published Bi-Monthly from December to August.

LONDON:

FLEETWAY PRESS, LTD.,
3-9, DANE STREET HIGH HOLBORN, W.C.1.

1924

THE PHYSICAL SOCIETY OF LONDON.

1924-25.

OFFICERS AND COUNCIL.

President.

F. E. SMITH, C.B.E., F.R.S.

Vice-Presidents.

(WHO HAVE FILLED THE OFFICE OF PRESIDENT)

SIR OLIVER J. LODGE, D.Sc., F.R.S.
SIR RICHARD GLAZEBROOK, K.C.B., D.Sc., F.R.S.
C. CHREE, Sc.D., LL.D., F.R.S.
PROF. H. L. CALLENDAR, M.A., LL.D., F.R.S.
SIR ARTHUR SCHUSTER, PH.D., Sc.D., F.R.S.
SIR J. J. THOMSON, O.M., D.Sc., F.R.S.
PROF. C. VERNON BOYS, F.R.S.
PROF. C. H. LEES, D.Sc., F.R.S.
PROF. SIR W. H. BRAGG, K.B.E., M.A., F.R.S.
ALEXANDER RUSSELL, M.A., D.Sc, F.R.S.

Vice-Presidents.

C. R. DARLING, F.I.C.
E. H. RAYNER, M.A., Sc.D.
T. SMITH, B.A.
J. H. VINCENT, D.Sc., M.A.

Secretaries.

PROF. A. O. RANKINE, D.Sc.
Imperial College of Science and Technology, South Kensington, S.W.7.

D. OWEN, B.A., D.Sc.
62, *Wellington Road, Enfield, Middlesex.*

Foreign Secretary.

SIR ARTHUR SCHUSTER, PH.D., Sc.D., F.R.S.

Treasurer.

W. R. COOPER, M.A., B.Sc.
82, *Victoria Street, S.W.*1.

Librarian.

J. H. BRINKWORTH, B.Sc.
Imperial College of Science and Technology.

Other Members of Council.

C. B. BRYAN, D.Sc.
PROF. C. L. FORTESCUE, O.B.E.
J. GUILD, A.R.C.Sc., D.I.C.
F. L. HOPWOOD, D.Sc.
E. A. OWEN, B.A., D.Sc.
R. W. PAUL.
J. ROBINSON, M.Sc., Ph.D.
PROF. S. W. J. SMITH, M.A., D.Sc., F.R.S.
J. S. G. THOMAS, D.Sc.
W. S. TUCKER, D.Sc.

Assistant Secretary (Publications)

CAPT. C. W. HUME, M.C., B.Sc.
14, *The Hawthorns, Finchley, N.*3.

VII.—ÆOLIAN TONES.

By E. G. Richardson, *M.Sc.*, East London College.

Received November 9, 1923.

Abstract.

The history and previous scientific work concerning vibrations maintained by fluid motion first described. New experiments for testing, by several methods, the dependence of the pitch n on the fluid speed V, and on the diameter D, of the vibrator in a number of fluids follow. The results show V/nD to fall from 8 to 5, while D ranges from 0·02 to 2 cm., whatever the viscosity. Results for square-section cords are also given, and a value of the critical speed for turbulence deduced.

Historical.

THAT sounds may be produced from wires strung up in the wind, or in the draught of the fire, has been known from the earliest age of musical instruments. Though there are various vague references to "harps" constructed on this principle, they seem to be a comparatively late invention, as the first description of one occurs in an early 17th century book by Athanasius Kircher,* who gives instructions for making one of these toys, which may be hung in a window or before a fire: "*In Æoliis pilis aer fervore ignis agitatus, insolitum quendam et harmonicum strepitum incitat.*" Here we have the first use of the present name for these sounds, from Æolus, god of the winds. Kircher had a vague idea that the variety of notes was caused by the wind blowing on different parts of the string. The early instruments consisted of a number of equal strings, tuned to unison on a sound-box. Langguth added a funnel to direct the wind, while Kaufmann conceived the ambitious plan of making an organ of the apparatus, adding a "pedal stop" of two thicker wires, giving the sub-octave of the others; but his attempt to add a "4-foot" was unsuccessful. Kaufmann made an important step when he made his wires of different thicknesses, but all tuned to the tone proper to the sound-box, thus ensuring greater probability of the wind-speed at any moment being one of those proper to a wire of a certain diameter (*cf.* theory below).

The æolian phenomenon is also known to us in the "singing" of telegraph wires, of the elastic stretched across the bars of a moving bicycle, and in the side movement of an oar drawn through the water.

The first experiments on the subject were those of Young,† who, having removed all the strings but one from an Æolian harp, proved Kircher's idea incorrect by finding that at the appropriate wind speed the string vibrated as a whole, no matter what part the draught impinged on. When the wind blew too rapidly for this to happen, the string might still vibrate in aliquot segments. The first quantitative investigation of the dependence of pitch on wind-velocity was made by Strouhal

* Musurgia Universalis (ii), 273, and Phonurgia, 148 (1620).

† Enquiry into the Prin. Phenon. of Sounds (1784).

in 1878.* The apparatus that he used was designed to whirl a wire rapidly through the air in a direction at right-angles to its length ; it was attached to the ends of two arms, the other ends of which were fixed to an axle, which could be turned (through gearing) by hand. It was possible so to turn the axle that the note remained the same long enough for the frequency of it to be estimated on a sonometer. The noise of the machinery and weakness of the tones prevented observations when the linear speed of the wire through the air was more than 5 m. per second. The facts that emerged from the results were that the frequency n (1) was independent of the length or the tension of the wire, (2) depended directly on the velocity V, (3) depended inversely on the diameter of the wire D. Strouhal put $n=\mathrm{K}/D\,[V-f(V)]$; the function $f\,(V)$ was to allow for any dragging of the air by the moving part of the apparatus, which was shown to be less at greater distances from the axle.

This Paper remained the only contribution to the study of the subject until 1914, when these results were taken up again by Krüger and Lauth,† and analysed in the light of some discoveries of Benard,‡ who, on examining the stream-lines behind an obstacle placed in running water found two parallel rows of alternate and equally spaced vortices, formed in the liquid, one on each side of the obstacle. The same effect was found by Borne§ in the air behind an obstacle. The production of these vortex-sheets these authors considered to be the source of the sound, and further pointed out that when the frequency of their production coincides with the natural frequency of the body behind which they are formed, the latter, if free to move, will be set into resonant vibration, augmenting the sound due to the vortices, an effect which Strouhal claimed to have found. Some later work on the lines of Benard was performed by Karman and Rubach,‖ who found from their photographs that h/l was a constant, independent of D and of V ; where h is the distance between the two rows of vortices, and l the length between two successive ones in the same row. Also l was approximately proportional to, and greater than, D, so that $l=bD$, say ; the velocity U of the vortex system relative to the (stationary) fluid was less than, but proportional to, V—i.e., $U=aV$. These facts led Krüger and Lauth to a theoretical basis for Strouhal's formula. The frequency of the vibration represents the number of vortices formed on one side of the wire in one second. If the body swings with the vortices, $\frac{l}{V-U}$ is the time between the disengagement of two successive vortices from the same side of the body—i.e., the period of swing :

$$\frac{1}{n}=\frac{l}{V-U}$$

$$\frac{V}{nD}=\frac{V}{D}\left(\frac{l}{V-U}\right)=\frac{l}{D}\left(\frac{V}{V-U}\right)=\frac{b}{1-a} \quad \ldots\ldots\ldots \quad (A)$$

which is a constant. From Karman and Rubach's results for a and b, Krüger and Lauth obtained the value 5·0 for this constant, which is in fair agreement with Strouhal's numbers, which lie between 6·3 and 4·9.

* Ann. der Phys., 5, 216 (1878).
† Ann. der Phys., 44, 801 (1914).
‡ Comptes Rendus, 147, 839 (1908).
§ Zeit. Flügtechnik, 3, 30.
‖ Phys. Zeitschrift, 13, 433 (1912).

The late Lord Rayleigh,* soon after this, pointed out that the vibration of the ire lay entirely in a plane perpendicular to the direction of the wind. He also st produced vibrations in bodies suspended in liquids. He set a bowl of water in tation about its centre, and set up a pendulum with its lower end dipping in the ater, so that it was free to swing along a line passing through the centre of the owl, and estimated the speed of the water, when the resonant swinging of the ndulum was a maximum. Lord Rayleigh, in addition, performed a few experi-ents on wires, held in the draught of the chimney-flue, but gave only a single pical result in his Paper, the velocity of the air-current being estimated by the ressure it exerted on a disc at the end of a pendulum, provided with a mirror. e found, however, considerable discrepancy between his results and those of trouhal, without being able to account for the difference, save by supposing the ragging of air by Strouhal's apparatus to have been greater than was assumed.

Since I commenced my series of experiments on the subject, which were originally ndertaken in the hope of finding the true value of this constant under different onditions, I have discovered two other pieces of work. One, by Relf,† is a repetition f Strouhal's experiment (without acknowledgment), for a few wires, "whirled" bout the axis of an electric motor; the other, by Krüger,‡ who doubted the accuracy f Lord Rayleigh's observations, and repeated his method for three pendulums uspended in one of the "straights" of an oval channel, round which water was irculated by a propeller. In the work which I describe below I have endeavoured o evaluate this constant for as large a range of diameters as possible, and by as many ethods as occurred to me. It is impossible to cover the whole range by one method, s thin wires require a different form of constraint from thick wires, or from rods, n order to get a good observable vibration. First, however, a résumé of the results btained and ranges covered by previous investigators will be given.

Fluid.	Air.	Air.	Air.	Water.	Water.
Name	Strouhal	Rayleigh	Relf	Rayleigh	Krüger
No. of Results	160	1	9	4	20
Diameters (cm.)	0·018 to 0·325	0·95	0·01 to 0·7	0·35 to 0·85	0·25 to 0·5
Average V/nD	5·9	(6·1)	7	6·6	5·2

For the thinnest wires Strouhal and Relf obtained higher values (6 and 8 respectively) of the constant. With the above is to be compared the value 5·0, calculated rom Karman and Rubach's photographs, for a cylindrical rod 1·5 cm. diameter mmersed in a moving liquid.

Results with Rods ("Tramway" Method).

The first series of my experiments were carried out with cylindrical rods, ranging n diameter between 1·5 to 5 cm. There was in the Physical Laboratory of East London College a miniature overhead railway, carrying a light trolley, which could be run along at speeds up to 6 ft. per second. It was thought that this would serve to carry a pendulum through the air at a known steady speed, which could be altered until a swing of maximum amplitude showed that the periodicity of the release of the vortices coincided with that of the pendulum. The rail on which the trolley

* Phil. Mag., 29, 433 (1915).
† Phil. Mag., 42, 173 (1921)
‡ Ann. der Phys., 60, 279 (1919).

ran was first levelled as perfectly as possible, and the trolley supported by an extra wheel running on a wire below, in order that the forward motion should be steady, and that there should be no vibration of the point of support of the pendulum. The latter was watched through a telescope, while the trolley ran over the central three metres of the track, the total run each time being about 10 metres, the pendulum being illuminated by an electric lamp fixed on the trolley. By electric contacts, a clock was started as the trolley ran on to the central section of the line, and stopped after the trolley had completed the three metres. The natural period of vibration of the pendulum was found while the trolley was stationary. A few results are given below, but it was by no means easy to distinguish the maximum swing, the amplitude produced being usually quite small.

TABLE I.—*Cylindrical Pendulums in Air.*

Material.	D (cm.)	n	V (cm./sec.)	V/nD
Card (hollow)	4·1	0·91	24	6·4
,, ,, ...	4·1	1·0	21	5·1
Paper...	3·9	0·8	25	8·0
,, ...	3·9	1·0	25	6·4
Wood	2·0	1·0	10·5	5·2
Paper	2·1	1·07	15	6·7
,, ...	2·1	0·94	14	7·1

It will be seen that recourse had to be made to pendulums of cardboard, and even of paper, to get a readable amplitude, and even then some means was wanting by which the vibrator could be examined more closely, so that this method was soon superseded by others to be described.

The same pendulums were also suspended in the wind-channel used for the wires (*vide infra*), but sufficiently small wind-speeds were not attainable.

ROTATING TANK METHOD FOR OBSERVATION IN LIQUIDS.

The third method used for rods was a modification of Lord Rayleigh's method. A circular tank about 4 ft. diameter had an inner concentric wall about 4 in. from its outer edge, enclosing an annular space, which was filled with water to a depth of about 8 in. The object of having only a ring of liquid, instead of filling the whole tank, was three-fold, (1) to prevent the liquid lagging behind the tank, when the latter was rotated, (2) to reduce weight, (3) for economy, when another liquid had to be used. The tank was fixed to a board, and had a peg beneath its centre, dipping into an oil-filled bearing. The outer edge of the board was supported on rollers. By means of a pulley-wheel under the tank, it was driven by an electric motor through double reduction gearing. A stroboscopic disc was mounted on the intermediate shaft (which was horizontal). By observing this disc through a stroboscopic vibrator, the speed of rotation of the tank could be found. This was compared with the time of revolution, as found by a stop-watch, but there was no detectable slip between the pulley, carrying the disc, and the pulley under the tank. Time was allowed for the water to attain the speed of the tank, in starting, but, with the wider rods suspended in the water, the speed of the latter never quite reached that of the tank, and in such cases V was estimated by timing little pieces of paper placed on the surface of the water at the same distance from the axis, as the pendulum, in the undisplaced position. The pendulums were supported so that

ney hung in the middle of the annular space, with an inch or so submerged. The naximum amplitude and frequency of the swing could be varied by altering the roportion of rod beneath the surface (this was adjusted to give a maximum mplitude of about 2 cm.) ; but the frequency was more readily increased by cutting ieces off the pendulum. The material in every case was light hollow brass tube, lugged at the lower end.

TABLE II.—*Pendulums in Water.*

D	n	V	V/nD	D	n	V	V/nD
0·95	1·54	8·7	5·95	2·54	1·33	18·5	5·5
0·96	1·54	8·9	6·1	2·80	1·25	20·5	5·85
1·30	2·50	20·0	6·15	2·80	1·05	13·8	4·7
1·30	1·29	9·5	5·7	2·80	0·91	15·0	5·9
1·30	2·10	15·0	5·5	3·85	1·33	22·5	4·4
1·60	2·20	18·5	5·25	3·85	1·05	22·2	5·5
1·90	1·22	12·5	5·4	4·10	0·90	19·4	5·3
1·90	1·25	12·5	5·25	4·10	1·33	24·0	4·4
1·90	1·82	19·5	5·65	4·80	1·57	28·6	3·8
...	...	...	...	4·80	1·53	24·0	3·3

RESULTS WITH WIRES AND CORDS (" WHIRLER ").

The first method I used for the investigation of the æolian tones of wires was trouhal's. The " whirler " rotated about a horizontal axis, to which two stout teel bars were rigidly attached, each having a little clamp at the further end to old the wire, and adjustable counterpoise weights on the opposite side of the axis o the wire. The little clamps were passed through holes in the steel bars, in which hey were held by thumbscrews, by means of which the tension on the wire, and, hereupon, its fundamental frequency, could be altered. Three pulleys of different izes were attached to the axle, also a stroboscopic disc, for estimating the speed f rotation. The wire was 45·7 cm. from the axis of rotation (nearly double that n Strouhal's apparatus), and as the axle was only 4 cm. thick, it was hoped that here would be little viscous drag of the air, especially as the long radius reduces he angular velocity necessary. The apparatus was driven by a long belt from a notor in an annexe, so that the noise of the motor was scarcely audible. With he radius stated, V was found by multiplying the number of revolutions per second by 286. The frequency of the note was found (after Strouhal) by tuning a sonometer o unison with it.

As mentioned earlier, Strouhal and others have held that the tones given out are produced in the air, i.e., by the mere formation of a regular series of vortices, but that when the frequency of their formation coincides with that of the wire, the latter " chimes in " resonantly, causing the sound to be amplified. I have been unable to detect any sound but that of the fundamental and harmonics of the wire, in spite of all precautions to keep the machinery silent, nor have others, whom I have asked to listen to the tones. I am therefore led to the conclusion that the so-called æolian tones are sounds *entirely produced by the wire itself*, though the vortex-formation is the driving force which keeps the wire in vibration.* For example, if one tunes the wire in the apparatus to the note c, and starts the motor with a

* To this extent the tones *are* dependent on the tension of the wire, in that they are all harmonics of the fundamental tone of the wire, which is regulated by the tension.

gentle acceleration when the appropriate velocity is reached, this note is first heard, reaches maximum intensity, and dies away again. The velocity steadily increasing, at about double the first speed, the octave c′ is heard ; again follows a period of silence until one hears the next harmonic, g′ ; then c″, and so on. For each note there is a range of resonance, whose extent is roughly proportional to V. As, however, the frequencies of the harmonics are proportional to the natural numbers, the resonance " peaks " of the higher harmonics will overlap ; there will then be no further periods of silence ; the sound will pass continuously through the whole series of harmonics (Fig. 1). And this is what actually happens ; sometimes, indeed, when the motor is adjusted to run at constant speed and, owing to a temporary change in the power, there is a slight alteration in V, the note is heard to jump

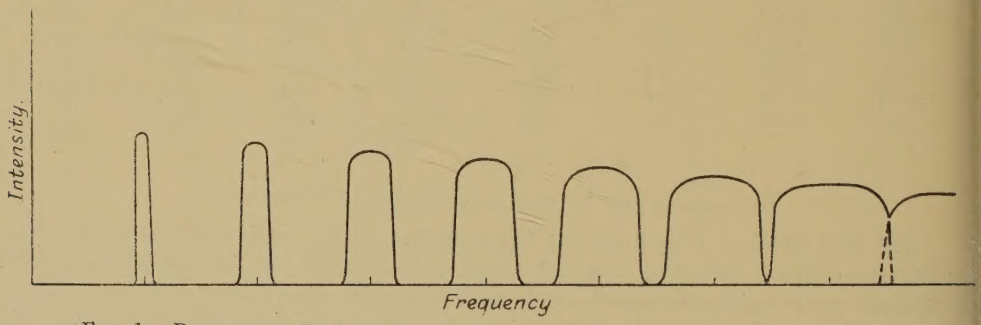


FIG. 1.—RESONANCE DIAGRAM OF THE AEOLIAN TONES OF A TYPICAL WIRE.

suddenly to the harmonic, next above or below ; and then back again, as the motor recovers.

In the following table are shown results for a number of steel wires, up to 0·06 cm. diameter. If thicker wires are used they must be loose to produce an audible tone, and then the tones given are weak, high harmonics, so that it is impossible to compare them with the sonometer. The theoretical values of the frequencies, i.e., those of the harmonics of the note, to which the wire had been previously tuned, are given side by side with the observed values, as estimated on the sonometer.

TABLE III.—*Wires in Air.*

D	n		V	V/nD	D	n		V	V/nD
	Theor.	Obs.				Theor.	Obs.		
0·025	1,536	1,530	405	10·5	0·03	2,175	2,175	634	9·7
0·025	2,048	2,020	465	9·2	0·03	3,625	...	950	8·7
0·025	3,560	3,600	633	9·75	0·03	2,048	1,960	486	8·3
0.025	1,460	1,460	405	11·1	0·03	2,560	2,660	705	8·85
0·025	2,280	2,210	480	8·65	0·03	3,072	3,160	755	7·95
0·025	2,920	2,930	558	7·6	0·05	720	720	358	9·95
0·025	3,650	3,500	790	9·1	0·05	1,240	1,230	538	8·65
0·025	1,660	1,720	353	8·2	0·05	677	677	356	10·5
0·025	2,075	1,960	390	7·5	0·06	1,536	1,520	476	5·2
0·025	2,600	2,550	476	7·5	0·06	1,024	1,020	323	5·3
0·03	725	725	190	8·75	0·06	623	623	323	8·65
0·03	1,450	1,410	356	8·4	0·06	1,246	1,250	430	5·75

Wind Tunnel Method.

It will be noticed that the value of V/nD tends to decrease, as the diameter of the wire increases ; the notes that were heard plainly were always of high frequency, and, therefore, difficult to estimate. A method was next devised, which should enable n and V to be determined more accurately, by removing the doubt whether the velocity measured was really that of the wire, relative to the surrounding air.

The idea of this method was to place a stationary wire in a current of air, instead of having the wire moving through a (more or less) stationary fluid. By the kindness of Dr. Piercy, I was enabled to set up my wire in a wind-channel in the Aeronautical Laboratory at East London College. The channel in question had a long glass window in one side, facing a smaller window in the opposite side. The wire was so placed in the channel that it could be observed, in a direction inclined about 20 degrees to the axis, through a telescope, near one end of the long window, the

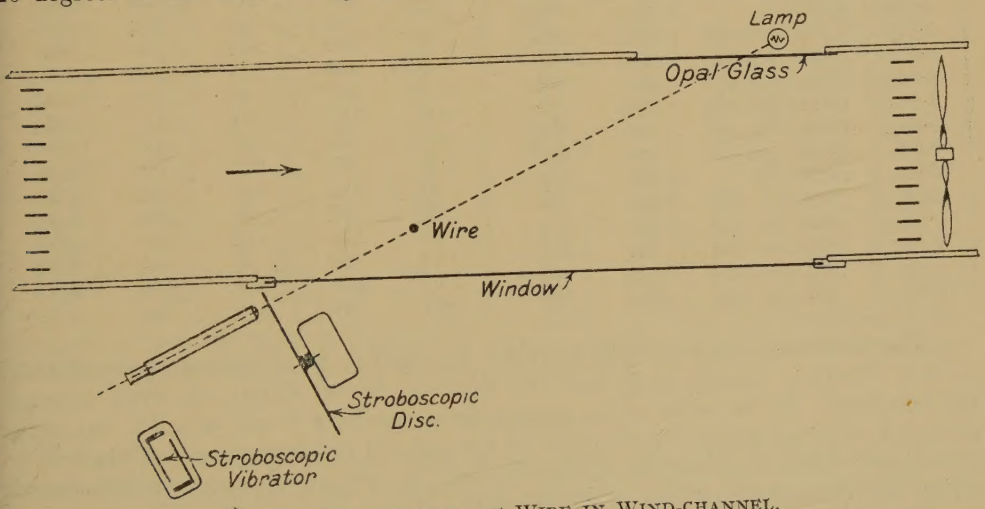


Fig. 2.—Arrangement of Wire in Wind-channel.

wire being illuminated by an opal-glass lantern placed behind the small window (Fig. 2).

As the noise of the fan and motor prevented all but the louder tones of the wire from being distinguished, the vibration of the wire was observed through the telescope, of short focus, the frequency being found by a stroboscope method, similar to that devised by the writer for finding the frequency of vibration of Trevelyan's Rocker.* A motor carrying a stroboscopic disc having twelve slits equally spaced round its circumference revolved directly in front of the object glass of the telescope, so as to cut off the light twelve times in each revolution. The speed of this motor was adjusted by means of a rheostat, until the *fastest speed* of the disc at which the vibrating wire appeared stationary was obtained. This speed in revs. per sec. was then known by looking through a stroboscopic vibrator, just beside the telescope, and the speed, multiplied by 12, gave the frequency of the wire independently of the ear or of any comparison of pitch. The velocity

* Phil. Mag., 45, 976 (1923).

of the air-current was found from a Xylol manometer which was calibrated from a Pitot tube, placed at the point where the centre of the wire would come in the actual experiment. The wires were fixed vertically in small clamps, as before, with adjusting screws for altering the fundamental frequency. For diameters greater than $\frac{1}{2}$ mm. rubber cords of circular section were used; they were about 30 cm. long between the clamps, and each end was about 10 cm. from the wall of the channel.

TABLE IV.—*Wires and Circular Cords in Air.*

D	n		V	V/nD	D	n		V	V/nD
	Theor.	Obs.				Theor.	Obs.		
0·020	1,020	1,020	210	9·55	0·195	380	375	385	5·25
0·020	1,530	1,560	280	9·0	0·195	190	192	190	5·15
0·020	2,040	2,020	360	8·9	0·21	304	304	315	4·95
0·020	2,550	2,650	405	7·65	0·28	210	212	320	5·4
0·020	3,060	...	450	7·35	0·32	180	178	305	5·4
0·025	1,400	1,400	265	7·55	0·32	300	300	520	5·4
0·025	1,750	1,750	320	7·3	0·435	128	127	310	5·6
0·025	2,100	1,970	360	7·3	0·435	192	189	450	5·45
0·03	1,024	1,025	315	9·3	0·44	168	174	415	5·6
0·03	1,536	1,540	385	8·15	0·44	224	220	515	5·3
0·03	2,048	2,060	490	8·0	0·44	280	276	640	5·3
0·03	2,560	...	560	7·3	0·47	152	150	380	5·4
0·06	269	269	215	8·0	0·89	90	90	450	5·6
0·06	538	538	260	8·2	0·89	135	133	670	5·65
0·09	270	274	175	7·1	1·14	90	94	610	5·7
0·09	405	405	260	7·2	0·07	258	258	163	9·0
0·09	540	540	340	7·0	0·07	387	387	225	8·2

The frequencies of the 0·025 and 0·03 wire had to be estimated by sonometer, as the vibration, though loud, was too small to be accurately observed through the telescope. In the series of harmonics given out by a single wire, the velocities observed were not strictly in the ratio of the natural numbers, but were such as to give higher values for V/nD for the lowest note, or two lowest notes in the harmonic series, and this independently of the absolute value of the velocity. The very loose resonance shown by wires and cords excited in this manner, i.e., by vortical turbulence, especially when vibrating in several aliquot parts, constitutes an unavoidable difficulty in determining the value of the velocity which best excites a particular tone, and renders it imperative to "strike a mean," in filling the third column; so that, throughout this work, V/nD is stated to the nearest 0·05 only. Two other points in the wind-channel experiments are worthy of note: (1) when a cord was sounding a harmonic, loops and nodes could be plainly seen through the telescope; (2) no vibration could be observed when the telescope was turned to view the cord in a direction at right-angles to the current.

The third series of experiments with wires consisted of an examination of their æolian vibrations in the revolving water-tank, for comparison with the figures obtained from vibrations in an air-current, given in the two preceding tables. The wires were held in a sort of hack-saw frame, with similar adjustable clamps. They were about 20 cm. long, and about one-third of their length was submerged in order that a fixed microscope could be focused on the mid-point of the wire, and just clear the revolving tank. The frequency was found by the same stroboscopic method, save that the disc revolved in front of the lamp, which shone on the wire,

rendering the illumination, instead of the view, intermittent. Using a micrometer eyepiece, it is possible by this method to measure the amplitude as well as the frequency of a vibration, if both are required. The wires of diameter greater than 1 mm. were clamped at one end only, in order to get a larger vibration. The frequency was then raised by cutting pieces off.

TABLE V.—*Wires in Water* (*Temp.* 15°—20°*C.*).

D	*n*	*V*	*V*/*nD*	*D*	*n*	*V*	*V*/*nD*
0·020	240	41·0	8·55	0·142	40·0	40·0	7·05
0·020	300	52·0	8·65	0·142	33·3	34·5	7·3
0·020	355	58·5	8·25	0·200	47·0	63·0	6·7
0·030	144	37·5	8·65	0·200	45·8	60·0	6·55
0·030	216	52·0	8·05	0·200	26·0	31·0	6·0
0·030	282	68·5	8·05	0·205	28·4	41·0	7·0
0·050	133	57·3	8·6	0·233	31·6	44·0	6·0
0·050	230	88·0	7·65	0·233	42·0	60·0	6·1
0·050	212	80·5	7·6	0·233	45·0	68·5	6·5
0·075	133	77·0	7·7	0·300	14·0	26·0	6·2
0·075	148	86·5	7·8	0·300	20·0	40·0	6·65
0·075	160	84·5	7·05	0·320	21·0	39·5	5·9
0·090	118	83·0	8·55	0·320	35·3	61·5	5·5
0·090	120	84·5	7·85	0·475	20·0	50·0	5·55
0·090	160	112·5	7·75	0·475	25·0	72·0	6·6
0·090	212	135·0	7·1	0·475	37·5	100·0	5·6
0·142	37·5	38·5	7·2	0·960	10·0	49·5	5·5
0·142	30	32·0	7·5	0·960	14·0	73·0	5·4

(Again, no vibration could be observed in a plane parallel to the direction of the stream even when there was quite a large vibration perpendicular to it.)

EFFECT OF VISCOSITY.

The above results show that there is very little difference between the aerodynamic and the hydrodynamic values of the constant. That this is to be expected follows from the observed fact that the frequency is proportional to V/D. This constant V/nD is a quantity of no dimension, so that if the viscosity ν (i.e., the dynamic viscosity which is relevant to this experiment) is to be introduced into the formula, another factor of equal dimension must be introduced to reduce the whole quantity to a numeric. For this additional factor, Lord Rayleigh put VD, his equation reading $V/nD=f(\nu/VD)$, assuming that the value of the quantity on the left would would alter if a fluid of different viscosity were introduced. He next expanded this function as a Maclaurin series, of which he took the first two terms, and applied them to Strouhal's results, finally obtaining an equation which satisfied these fairly well, yet retaining the viscosity term :

$$nD/V=0{\cdot}195\left(1-\frac{20{\cdot}1\nu}{VD}\right).$$

It must be understood that this formula was made to fit (by putting $\nu=0{\cdot}15$) results for air only. And when the values of ν/VD in the table which follows are substituted in this formula, the right-hand expression assumes a much larger range of variation, by change of viscosity, than is covered by the experimental values of nD/V.

The actual study of the effect of viscosity on the tones was made with wires

and rods in the same tank. The viscous liquid was composed of molasses, diluted with water; but, starting from the highest viscosities attainable, and reducing it step by step, no vibration was visible until $\nu=0{\cdot}65$ was reached. Viscosity was measured by an Ostwald form of viscometer, standardised with water. In the viscous liquids, when the speed was correctly adjusted, resonance was not as strong nor as sharp as in air and in water, consequently it was more difficult to judge when the best resonance had been obtained.

TABLE VI.—*Wires in Viscous Mixture.*

D	ν	n	V	V/nD	VD/ν
0·083	0·155	123·0	80·0	7·65	42·8
0·083	0·084	150·0	112·0	7·65	110·0
0·090	0·084	141·0	120·0	9·5	129·0
0·090	0·155	144·0	106·0	8·2	62·5
0·20	0·084	23·6	38·0	8·05	90·5
0·20	0·155	40·0	58·5	7·3	75·5
0·20	0·23	25·0	37·0	7·4	32·2
0·20	0·23	37·5	60·0	8·0	52·3
0·30	0·084	26·0	59·5	7·6	209·0
0·30	0·084	46·0	95·0	6·95	340 0
0·30	0·155	23·0	51·0	7·4	99 0
0·30	0·155	45·0	102·0	7·55	197·0
0·30	0·23	27·0	60·5	7·5	79·0
0·30	0·23	48·0	95·0	6·6	125·0
0·32	0·084	18·7	45·0	7·5	171·0
0·32	0·155	18·7	44·0	7·35	87·8
0·41	0·23	19·8	48·5	5·85	101·0
0·47	0·084	37·5	98·5	5·6	543·0
0·47	0·155	37·5	98·5	5·6	294·0
0·47	0·23	35·0	94·0	5·7	190·0
0·47	0·46	39·0	100·0	5·5	102·0
0·55	0·23	22·5	60·0	4·85	143·0
0·57	0·65	15·0	44·5	5·2	38·5
1·15	0·65	15·0	120·0	5·8	177·0

These results are shown in Fig. 3, side-by-side with two curves representing the trend of the average water and air results.

The variation of V/nD must be small, if any, as it does not show itself in a comparison of the last table with the earlier ones, which deal with air and water. Also, as far as may be judged, this quantity is independent of VD/ν—e.g., taking a rod of $\frac{1}{2}$ cm., the latter term may run from circa 300 for a viscosity of 0·155 to 3,160 in water of one-tenth this viscosity, without more than a fractional change in V/nD. The only records of change in the position of the vortices with viscosity, and those very scanty, have been given by Benard, who estimated (by what means is not stated) that when the viscosity increased sevenfold, l increased in the proportion 1 : 1·4, with constant D. Whether this change of $b(=l/D)$ is accompanied by a change in the other important quantity $a=U/V$, Benard did not state; the only observations of the movement of the vortex-system as a whole, made by Rubach, were in water, so that ν was constant. It is likely, however, that this quantity also changes with ν, as Benard noted that the speed of rotation of the vortices increased in greater viscosities, which with their increased distance apart would alter their

translational velocity. But it is not possible, in default of measurements of a and b in viscous liquids, to say to what extent, if any, the theory would require V/nD to change with ν.

Effect of Shape.

In the very first set of experiments with pendulums, it had been observed that square-section rods gave considerably higher values of the constant than round ones, but these results were passed over at the time, and all the foregoing observations apply to vibrators of circular section only. I now returned to the square

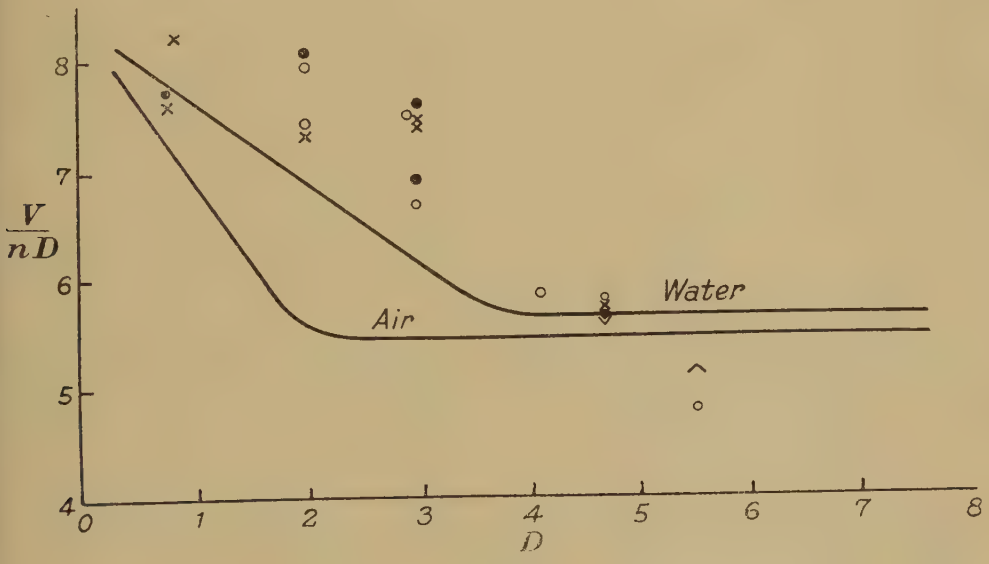


Fig. 3.

(The numbers on the horizontal scale here represent mms.).

pendulums, and obtained results for a few of these in water, and for rubber cords of square section in the wind-channel. The widths do not extend below 2·2 mm., obtained by stretching a 4 mm. cord.

Table VII.—*Square Pendulums.*

In Air.				In Water.			
D	n	V	V/nD	D	n	V	V/nD
1·7	1·0	16	9·4	1·9	1·33	24	9·5
2·1	0·87	18	9·9	1·9	1·00	20	10·5
2·6	1·03	28	10·4	1·9	1·55	30	10·2
4·0	1·66	71	10·5	2·8	0·95	30	11·2
4·0	2·05	46	10·9	2·8	0·83	28	12·0

TABLE VIII.—*Square Cords in Air.*

D	n	V	V/nD	D	n	V	V/nD
0·22	105	280	12·1	0·34	219	780	10·45
0·22	215	555	11·7	0·36	101	405	11·1
0·22	320	830	11·75	0·36	202	810	11·1
0·29	104	360	11·9	0·40	82	360	11·0
0·29	206	670	11·2	0·40	164	710	10·8
0·34	73	310	12·45	0·48	135	680	10·5
0·34	146	580	11·7	0·48	190	940	10·3

In the formula (A) in the introduction to this Paper, it is shown how the value of V/nD depends on the vortex quantities l/D and U/V, which have been found to be constants for one form of obstacle. The fact that V/nD is doubled, when we employ "square" instead of "round" obstacles, points to a change in either or both of these, when the form of the obstacle is changed. Confirmation of this is obtained from the experimenters cited, who, although they did not use a square-section obstacle, examined the orientation of vortices behind a cylinder and a plate, with the following results :—

Cylinder : $D=1{\cdot}5$ cm., $l=6{\cdot}4$ cm., $b=l/D=4{\cdot}3$, $a=U/V=0{\cdot}14$.
Plate : $D=1{\cdot}75$ cm., $l=9{\cdot}8$ cm., $b=5{\cdot}5$, $a=0{\cdot}20$.
Substituting these values of a and b in the formula (A), we obtain for V/nD :—
Cylinder : 5·0. Plate : 7·0.

The size of these obstacles prevents them from being used in a vibration experiment, to determine their V/nD, but the proportionate increase (5·0 to 7·0) is close to that obtained experimentally (7 to 10) for round and square cords of less diameter, and, to the extent that a plate may be considered as a square rod for these purposes, explains the higher numbers for these latter.

COMPARISON OF RESULTS.

In the following table are shown the average results, obtained for each small range of thickness by the different methods (Circular Sections only).

TABLE IX.

D	Air (Whirler).	Air (Wind-chan.).	Water.	Viscous mixture.
Up to 0·03	8·8	8·2	8·3	...
0·03 to 0·07	7·7	8·1	8·0	...
0·07 to 0·1	...	7·1	7·7	8·2
0·1 to 0·3	...	5·4	6·7	7·4
0·3 to 0·4	...	5·4	5·7	7·4
0·4 to 1	...	5·4	5·6	5·6
1·0 to 2·6	...	...	5·6	...
2·6 to 4·0	...	...	5·2	...
4·0 to 5·0	...	...	4·2	...

The main difference in the effects of these fluids on the vibrations appears when D goes from 0·3 to 1 cm. ; in air, V/nD drops more rapidly to 5·4, than it does when liquids are in question. This is not a viscosity effect, as the same steady fall is exhibited in the molasses mixture.

Reynolds' Number.

In Table VI. the values of VD/ν are worked out. This quantity is of importance in the dynamics of fluids, as it was discovered by Osborne Reynolds* that, when a liquid flows through a tube of bore D, there will be a steady motion, and, consequently, no vortices will be formed, unless VD/ν exceeds a certain amount. By the principle of similarity, the same will be the case in the flow behind an obstacle in a stream, although the limiting value of VD/ν will not of necessity be the same as in the flow through tubes. It was no object of this research to evaluate this limit, but as it has not been determined in liquids of different viscosities hitherto, the numbers have been appended. Such were also given by Relf, who puts the limit at 150 (only for water experiments), but when VD/ν is down to 40 I still get a vibration. These facts explain the impossibility of getting a vibration in the higher viscosities, and also the fact that the fundamental Æolian Tone of a thin wire is usually not produced,† the appropriate value of V being too low to make VD greater than 33ν. A few cases where tones *failed* to be heard are appended, to show that the failure is accompanied by values of VD less than 34ν. In Table VI., VD/ν exceeds 33 in every case save one, where it is just below—32·2.

Table X.—*Theoretical Tones, which were not Produced.*

ν	D	n	V	VD/ν	ν	D	n	V	VD/ν
0·23	14·0	30	(23)	14·0	0·15 (Air)	0·025	1,050	(200)	33
0·155	0·05	115	(43)	14	0·15	0·02	1,100	(180)	24
0·46	0·2	26	(28)	12	0·15	0·058	900	(420)	16
0·46	0·3	24	(40)	26	0·15	0·07	129	(70)	33
0·65	0·2	46	(110)	33					
1·47	0·3	46	(95)	19·5					

(The numbers in the third column are calculated from the values of V/nD, which wires of the same size give in vibration.)

I am much indebted to Professor C. H. Lees, D.Sc., F.R.S., for many helpful suggestions and kindly criticism, both during the progress of this work and in the setting-out of the results.

DISCUSSION.

Dr. F. Ll. Hopwood congratulated the author on the quantitative results obtained in the face of great experimental difficulties. He referred to the author's conclusion that the pitch of æolian tones is not greatly affected by changes in the viscosity of the fluid. In repeating some of the experiments of Lord Rayleigh and Strouhal he had found that the pitch varies with temperature in a way that might be connected with changes of viscosity due to temperature. The author's suggestion that the sound came from the wire and not directly from the vortices was not confirmed by an experiment of his own in which a gas-supplied tube, perforated to give luminous jets, was rotated about a parallel axis in place of Strouhal's wire. In this case the occurrence of the critical or turbulent state could easily be observed by watching the gas jets, and the change of tone on reaching this state was very marked ; while the tube itself was too stiff to emit audible sounds in the circumstances.

Dr. W. S. Tucker said that if a wire be heated by an electric current so as to form a hot-

* Phil. Trans., p. 935 (1883).

† To simplify the argument, this fact is ignored on page 7.

wire microphone, connected through an amplifier to a receiver, and rotated, a musical note is produced in the receiver at certain speeds, in consequence of the effect of the transverse vibrations of the wire in causing variation of convectional heat-losses. At the critical velocity the musical note breaks down into a crackle. If the resistance of the wire be plotted against its speed relative to the air, a regular curve is obtained until the critical velocity is reached ; thereafter the relation becomes irregular and unstable.

Dr. C. V. Drysdale added his congratulations on the author's success in a difficult research. The absence of the fundamental from the series of tones appeared to suggest that the phenomenon was akin to that of the oscillating thermionic valve, in which the oscillation depended on the departure of the characteristic from the linear law. The resistance of a fluid to motion was nearly linear at low velocities, but became practically parabolic as the speed increased, so that it was reasonable to expect that oscillations would not commence until the speed exceeded the limit of the linear law, and this appeared to be confirmed by the connection between oscillation and turbulence. If the force on the wire could be measured at different speeds, it would probably throw a good deal of light on the theory.

While appreciating the difficulty of making satisfactory photographic records of the vortices, he suggested that if the author were to employ a convergent beam of light focused on the diaphragm of the photographic lens but just outside its aperture, a dark field would be obtained in which the smallest curvature of the surface would appear as a bright area ; and he thought that in this manner very clear photographs of the vortices might be obtained.

Mr. A. H. Davis suggested that the quantity V/nD should be plotted against the non-dimensional quantity VD/ν, instead of against D as in Fig. 3. As it is, three different curves are obtained for water, air and viscous liquids, whereas (unless some factor has been left out of account) by plotting non-dimensional quantities identical curves should be obtained. Dimensional considerations show that the quantities V, n, D and ν cannot be arranged in an equation in any way not reducible to Lord Rayleigh's form $V/nD=f(VD/\nu)$. If therefore one curve drawn to such co-ordinates did not serve for all cases, it would follow that some factor additional to those considered by the author must be taken into account. The speaker had tried plotting some of the results given in the Paper in the manner described, and the agreement for different fluids was fairly good except, perhaps, in the case of water (Table V). On the other hand Relf's results for water agree fairly well with the author's results for air and viscous mixtures. It seemed important to use non-dimensional quantities for both co-ordinates in order to bring out as fully as possible the theoretical bearing of the results. The vortices described in the Paper appeared to be of interest in connection with the stresses experienced by wires in an air stream. In studying these it is usual to plot a non-dimensional force factor against VD/ν, but it seems possible that some property not accounted for is responsible for certain irregularities (Bur. Stds. Sci. Paper 394).

Mr. F. E. Smith said that the Paper described a number of very difficult experiments. He pointed out that there seemed to be no justification for drawing part of the curves in Fig. 3 horizontal.

Author's reply (communicated) : The pitch of the note would certainly change with temperature ; for instance, by heating it with an electric current, as Dr. Hopwood suggested, but I should ascribe this change of pitch to a change in the elastic constants of the wire with temperature —i.e., to a change in the *natural* tone of the wire. And I venture to predict that such change would be accompanied by a corresponding change in V, leaving V/nD practically unaltered. Strouhal considered his results from temperature change insufficient. In this connection I should like to quote Lord Rayleigh's remark on his pendulum experiment : " An experiment to try whether change of viscosity had any appreciable influence (was tried), using water at 60°C. and at 12°C. No difference of behaviour was detected." In this case, the natural period of the vibrator would be practically unaffected by the change of temperature. In Dr. Hopwood's gas-tube experiment, I do not understand what produced the tone before the turbulent state was reached. I have failed to hear musical tones from " whirled " rods which were too thick and stiff to vibrate themselves.

I am grateful to Dr. Drysdale for his suggestion to improve the photography of vortices. By noting the deflection of the free end of a wire in the direction of the stream (of water), I found that the force on the wire due to the fluid was roughly proportional to V^2 above the critical velocity, as the theory predicts. The force was too small, however, to produce measurable deflections below this speed.

Like Mr. Davis, I had tried plotting V/nD against VD/ν, but could obtain no useful information concerning the form of the function $f\left[\frac{VD}{\nu}\right]$, for the points lie on a line—or rather, on a band—parallel to the VD/ν axis. In fact, the greater part of Relf's curve, from $VD/\nu = 500$ to 5,000, is a similar horizontal line, showing the two quantities to be independent. After all, V/nD is itself non-dimensional, and the principle of similarity does not require one to drag in the viscosity. I agree that there are irregularities difficult to explain, but I think that the cause is to be looked for in the properties of the wire, or other vibrator, rather than in those of the fluid.

I am glad Mr. F. E. Smith drew my attention to Fig. 3, wherein I apologise for a mistake. I have not extrapolated beyond my results; the unit on the horizontal scale represents a millimetre, whereas elsewhere the centimetre is used as unit in the Paper. I have added a note below the figure to that effect.

VIII.—THE EFFECTS OF TORSION ON THE THERMAL AND ELECTRICAL CONDUCTIVITIES OF METALS.

By J. E. Calthrop, *B.A., B.Sc.*

Received November 7, 1923.

Abstract.

A method which has been described previously and used for the determination of the changes in the thermal conductivities of metal wires, is used for brass, iron, zinc, nickel and tin.

As in the earlier experiments the small decreases obtained on twisting are proportional to the square of the twist, as is approximately the case for the electrical conductivities.

Iron has been most closely studied. It appears to require several days to return to its original value of the thermal conductivity after the twist has been removed, and if the twist is kept constant there is a slight return towards the initial value.

I.

THE effect of torsion on the electrical conductivities of metals has been determined by a number of observers, to whose work references will be found in a previous Paper. The effect on the thermal conductivities has received much less attention for the only work in this connection appears to be that of Smith* and that of Lees and Calthrop.†

Smith, experimenting on bars of iron, steel, copper and brass, concluded that the small decreases found were proportional to the twist. Lees and Calthrop, experimenting on wires of steel, copper, aluminium and lead found small decreases, which were more nearly proportional to the square of the twist.

The decreases in the electrical conductivities were smaller than the corresponding changes in the thermal conductivities.

In the present Paper the results obtained for iron, brass, nickel, zinc, and tin are described. The method employed for the experiments on thermal conductivity was that due to Professor C. H. Lees, by which a change of one part in a thousand may be detected with accuracy.

II. Outline of the Method.

The wire to be tested for thermal conductivity is held by two clamps in a water-cooled chamber. Heat is supplied to the centre of the wire by means of a current in a manganin coil. The temperatures at two points on the wire to be tested are obtained by means of platinum thermometers, wound on the wire.

The twist is applied by rotating one of the clamps and the change in the thermal conductivity may be found from the changes in the resistances of the platinum thermometers. Further experimental details of the apparatus and methods of observation will be found in the previous Paper.

* N. F. Smith, Phys. Rev., 6, p. 429 (1909).
† Lees and Calthrop, Proc. Phys. Soc., 35, p. 225 (1923).

III. Theory of the Method.

It has been shown that

$$H=(qka\cosh ax_0+hs\sinh ax_D)\cdot\frac{v_B-v_A}{\sinh ax_B-\sinh ax_A}\cdot\frac{ab}{\sinh ab} \quad . . . (3.1)$$

where H is the total supply of heat to half the wire, q is the area of the section of the wire, $a^2=ph/qk$, where p is the perimeter of the wire, h its emissivity, and k its thermal conductivity. x_0 is the distance of the near end of the heating coil from the clamp, x_D is given by the equation $x_D=x_0+\frac{1}{3}(x_c-x_0)$, where x_c is the distance of the centre of the heating coil from the clamp. v_B and v_A are the temperature excesses at distances x_B and x_A from the clamp. s is half the area of the wire, covered by the heating coil. b is half the portion of the wire covered by each of the platinum thermometers.

H may be expressed in terms of the resistance S of the heating wire, and the current A amperes through it, by the equation

$$H=\frac{SA^2}{4{\cdot}18} \quad . . . . . (3.2)$$

The temperature excesses v_B and v_A may be expressed in terms of the resistances of the platinum thermometers at the temperature of the case, and during the passage of heat. It was found that

$$v_B-v_A=\frac{35}{34}\cdot\frac{1}{\gamma}\cdot\left(\frac{R_1}{R_0}-\frac{r_1}{r_0}\right) \quad . . . . . (3.3)$$

where R_0, r_0 are the resistances before, and R_1, r_1 the values during the passage of heat. γ is the temperature coefficient of platinum calculated from the resistance at the mean case temperature, not on the resistance at 0°C.

From equations (3.1), (3.2), (3.3) it is found that

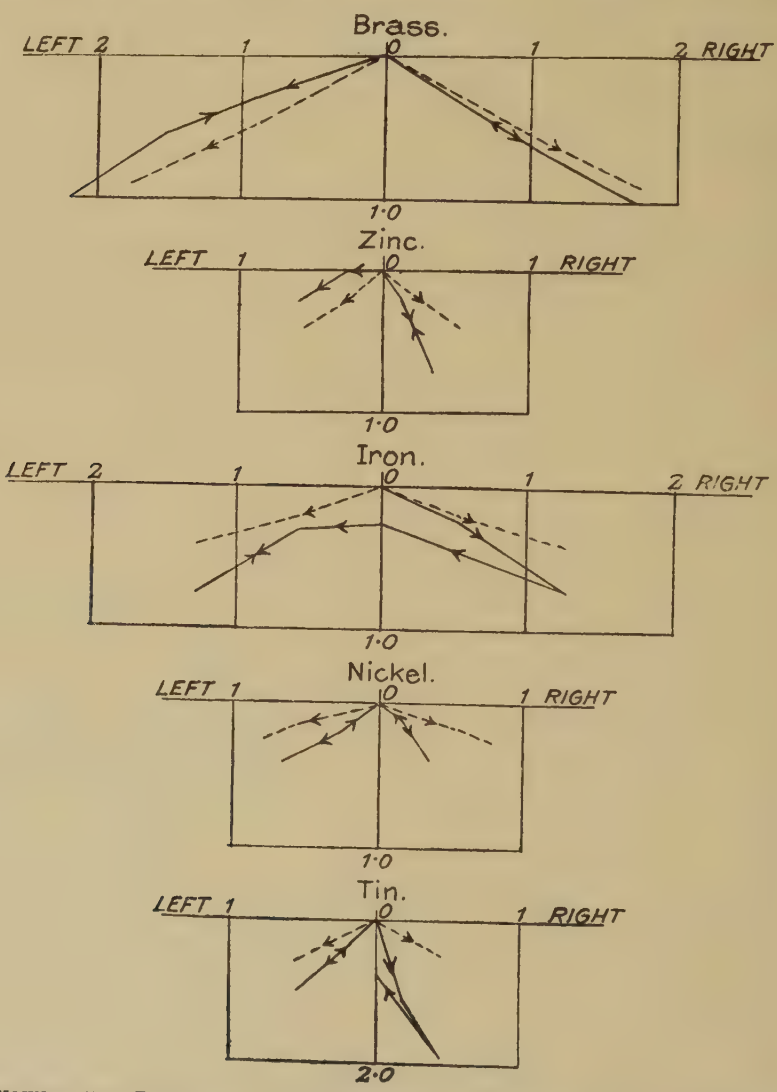
$$\left(qk+hsx_D\cdot\frac{\sinh ax_D}{ax_D}\cdot\frac{1}{\cosh ax_0}\right)\cdot\frac{a\cosh ax_0}{\sinh ax_B-\sinh ax_A}\cdot\frac{ab}{\sinh ab}=\frac{\gamma SA^2}{4.34\left(\frac{R_1}{R_0}-\frac{r_1}{r_0}\right)} \quad (3.4)$$

A simple method of finding the value of k, which corresponds to the measured value of $\left(\frac{R_1}{R_0}-\frac{r_1}{r_0}\right)^{-1}$ is to select a suitable value of k from physical tables, and calculate the value of $\left(\frac{R_1}{R_0}-\frac{r_1}{r_0}\right)^{-1}$ from equation (3.4).

A few trials will give suitable limits between which the experimental value lies and these values may be plotted on a graph. The actual value of k may then be found and also the percentage changes in the thermal conductivity for the observed changes in $\left(\frac{R_1}{R_0}-\frac{r_1}{r_0}\right)^{-1}$. The term $\frac{ab}{\sinh ab}$ differs little from unity. The emissivity h has been taken as 0·0003.

IV. Methods of Observation.

In the experiments on thermal conductivity the value of the conductivity was first determined without a twist being applied. Then a twist of 5—10° was put on and after about an hour, which was usually the time allowed for a steady state to be reached, the conductivity was re-determined. A further twist was applied,



Graphs showing the Decreases of the Thermal and Electrical Conductivities as a Result of Torsion.

Note.—Abscissæ represent the squares of the twist, which is expressed in degrees per cm., and the ordinates represent the decreases as percentages of the original conductivity. The full lines are for the thermal and the dotted for the electrical conductivities.

nd the process repeated. Finally the twist was removed, and the conductivity gain found, usually on the same day or the day following. Both right and left nded twists were given.

The electrical conductivity was measured by the potentiometer method, the nange in the fall of potential being measured while a constant current was sent rough the wire.

Two further points of interest were investigated in regard to iron.

In previous experiments it had been found that for fairly large twists the thermal nductivity did not regain its original value immediately after the twist had been moved. This was especially so for steel. In the present series of experiments on shows the same behaviour and readings taken at intervals show the gradual turn to the initial value.

Von Szily* as early as 1899 found that in the case of constantan after the elec-ical conductivity had decreased on twisting, it slowly increased towards its old alue, although the twist was kept constant.

In the present experiments a series of readings was made for iron during espe-ally steady temperature conditions to see if the thermal conductivity showed any rresponding effect. The experiments appear to show that there is a slight return ring constant twist.

V. Tables of Results.

1. Brass wire, 0·202 cm. in diameter, 13·6 cm. long, from London Electric Wire Co.

Heat Conductivity.

$x_B = 5{\cdot}4$ cm. $x_A = 1{\cdot}1$ cm. Heating current $= 0{\cdot}27$ amps.

k from equation (3·4) $= 0{\cdot}28$.

Date.	Twist. (Deg. per cm.)	(Twist)^2.	Conductivity.	Per cent. Decrease.
23/3/1	°	°	1·0000	...
	1·03 R	1·06	0·9935	0·65
	1·32 R	1·74	0·9900	1·00
23/3/2	°	°	1·0000	...
23/3/8	°	°	1·0000	...
	0·95 L	0·90	0·9970	0·30
	1·23 L	1·51	0·9945	0·55
	1·47 L	2·16	0·9900	1·00
23/3/14	°	°	1·0000	...

Electrical Conductivity.

23/10/16	°	°	1·0000	...
	0·96 L	0·91	0·9950	0·50
	1·32 L	1·75	0·9910	0·90
	°	°	1·0000	...

* Von Szily, Journ. de Physique, 8, p. 329 (1899).

2. Zinc wire, 0·200 cm. in diameter, 13·6 cm. long, from London Electric Wire Co.

Heat Conductivity.

$x_B = 5{\cdot}6$ cm. $x_A = 1{\cdot}1$ cm. Heating current = 0·27 amps.
k from equation (3·4) = 0·26.

Date.	Twist. (Deg. per cm.)	(Twist)^2.	Conductivity.	Per cent. Decrease.
23/3/19	°	°	1·0000	...
	0·37 R	0·14	0·9980	0·20
	0·59 R	0·35	0·9930	0·70
23/3/22	°	°	1·0000	...
23/4/17	°	°	1·0000	...
	0·51 L	0·26	1·0000	°
	0·74 L	0·55	0·9980	0·20
	°	°	0·9995	0·05

Electrical Conductivity.

23/10/22	°	°	1·0000	...
	0·44 L	0·19	0·9985	0·15
	0·76 L	0·54	0·9960	0·40
	°	°	1·0000	...

3. Iron wire, 0·200 cm. in diameter, 13·6 cm. long, from Johnson, Matthey & Co.

Heat Conductivity.

$x_B = 5{\cdot}4$ cm. $x_A = 1{\cdot}1$ cm. Heating current 0·3 amps.
k from equation (3·4) = 0·18.

Date.	Twist. (Deg. per cm.)	(Twist)^2.	Conductivity.	Per cent. Decrease.
23/5/23	°	°	1·0000	...
	0·74 R	0·55	0·9975	0·25
	1·13 R	1·28	0·9930	0·70
23/5/24	°	°	0·9975	0·25
	0·74 L	0·55	0·9970	0·3
	1·13 L	1·28	0·9925	0·75
23/5/31	°	°	1·0000	...

Electrical Conductivity.

23/10/15	°	°	1·0000	...
	0·74 L	0·55	0·9980	0·20
	1·13 L	1·28	0·9960	0·40
	°	°	1·0000	...

NOTE.—In all the experiments the measurements were made on the portion of the wire in which the heat was flowing towards the fixed end, but for iron a test was made on the portion in which the heat flowed towards the twisted end. A twist of 1·40° per cm. or a (twist)^2 of 1·96 gave a decrease of 1·45 per cent. in fair agreement with the above.

4. Nickel wire, 0·165 cm. in diameter, 13·6 cm. long, from London Electric ire Co.

Heat Conductivity.

$x_B = 5{\cdot}40$ cm. $x_A = 1{\cdot}20$ cm. Heating current $= 0{\cdot}21$ amps.
k from equation (3·4) $= 0{\cdot}16$.

Date.	Twist. (Deg. per cm.)	(Twist)^2.	Conductivity.	Per cent. Decrease.
23/6/21	°	°	1·0000	...
	0·37 R	0·14	0·9990	0·10
	0·59 R	0·35	0·9960	0·40
	°	°	0·9980	0·20
23/6/27	°	°	1·0000	...
	0·51 L	0·27	0·9980	0·20
	0·81 L	0·66	0·9960	0·40
23/6/28	°	°	1 0000	...

Electrical Conductivity.

23/10/22	°	°	1·0000	...
	0·74 L	0·55	0·9985	0·15
	0·89 L	0·78	0·9975	0·25
	°	°	1·0000	...

5. Tin wire, 0·200 cm. in diameter, 13·6 cm. long, from Messrs. Johnson, Iatthey & Co.

Heat Conductivity.

$x_B = 4{\cdot}8$ cm. $x_A = 1{\cdot}1$ cm. Heating current 0·21 amps.
k from equation (3·4) $= 0{\cdot}12$.

Date.	Twist. Deg. per cm.	(Twist)^2.	Conductivity.	Per cent. Decrease.
23/7/10	°	°	1·0000	...
	0·44 R	0·19	0·9890	1·10
	0·66 R	0·44	0·9810	1·90
	°	°	0·9930	0·70
23/7/12	°	°	1·0000	...
	0·44 L	0·19	0·9965	0·35
	0·74 L	0·55	0·9900	1·00
23/9/10	°	°	1·0000	...

Electrical Conductivity.

23/9/20	°	°	1·0000	...
	0·59 R	0·35	0·9975	0·25
	0·88 R	0·77	0·9955	0·45
	°	°	1·0000	...

Results for Iron, showing the Lag Effect of the Thermal Conductivity and the Gradual Return after the Removal of a Twist.

Date.	Time after Removal of Twist.	Resistance of Case Coil in ohms.	Twist in Deg. per cm.	$\left(\frac{R_1}{R_0}-\frac{r_1}{r_0}\right)^{-1}$	Conductivity.
23/5/28	...	2·2290	°	13·890	1·0000
	...	2·2255	°	13·890	
	...	2·2294	1·13 L	13·790	0·9925
23/5/29	21 hrs.	2·2291	°	13·850	0·9970
	...	2·2288	°	13·845	
23/5/31	70 hrs.	2·2280	°	13·880	0·9990
	...	2·2279	°	13·870	

Results for Iron, showing a Slight Return of the Thermal Conductivity towards the Original Value, under Constant Twist.

Date.	Resistance of Case Coil.	Twist.	$\left(\frac{R_1}{R_0}-\frac{r_1}{r_0}\right)^{-1}$	Conductivity.	Per cent. Decrease.
23/6/4	2·2280	°	13·89	1·0000	...
	2·2308	1·13°R	13·82	0·9945	0·55
	2·2297	1·13°R	13·81		
23/6/5	2·2263	1·13°R	13·85		
23/6/6	2·2300	1·13°R	13·86	0·9970	0·30
	2·2297	1·13°R	13·86		
23/6/7	2·2283	1·13°R	13·85		
23/6/8	2·2307	°	13·89	1·0000	...

NOTE.—In the resistance of the case coil one in the fourth place of decimals represents 0·012°C. and the maximum variation from the mean is about 0·3°C.

VI. Discussion of Results.

The small decreases obtained in the thermal and electrical conductivities of brass, zinc, iron, nickel and tin on twisting these metals, which were in the form of wires, appear to be very well represented as proportional to the squares of the twists. In most experiments the changes in the electrical are smaller than the corresponding changes in the thermal conductivities. In the case of brass it is difficult to draw a definite conclusion on this point.

After a wire has been twisted once it is very rarely possible to repeat the same curve exactly, and therefore in the graph there is not usually symmetry for right and left-handed twists.

As had been found previously, the thermal conductivity does not always return immediately to its original value after the removal of a large twist. This was particularly so for steel, and in the present series iron shows a similar behaviour. The electrical conductivities, on the other hand, appear to return quite readily after the removal of the twist.

For iron there appears to be a slight return of the thermal conductivity towards its original value, although the twist is kept constant.

This work has been done in the Physical Laboratory of the East London College, where Professor C. H. Lees, by his kindly interest and advice, has rendered assistance which is most gratefully recognised.

DISCUSSION.

Prof. A. O. RANKINE inquired whether any answer could now be given to the question ich he had put on a previous occasion—viz., whether the change of conductivity depends in y way on the sense or direction of the twist. He also inquired whether the stress had been rried up to the elastic limit, and pointed out that the fatigue of the specimens was bound up th their crystalline structure. It had recently been stated that it is possible to make crystals tungsten a quarter of a mile long; by working with a single crystal it might be possible to tain further information.

Mr. F. E. SMITH added that Prof. Carpenter had made crystals of aluminium several inches ng, and suggested that the author might relate his results to the changes in length and cross-ction of the wire under torsion.

Dr. T. BARRATT (communicated): The author does not commit himself to any theory to plain the changes in thermal and electrical conductivities when the wire is twisted. It would pear possible that a change in the sectional area of the wire on twisting might of itself produce anges of the order found experimentally. But a more probable cause is to be expected in e re-arrangement of the metallic crystals of the wire. In a drawn wire these crystals would mainly with their longer ones along the length of the wire. On twisting the wire the crystals ould tend to be displaced in such a way that the current (thermal or electric) would flow through em more or less diagonally. The effect would then correspond to the differences of conduc-vity found in a crystal in different directions. A similar theory might explain Johnstone's* sult that the thermal conductivity of a metal increases slightly when the metal rod is stretched ong its length. In this case the effect of stretching would be to place more of the crystals with eir longer—and presumably better conducting—axes along the length of the wire, and thus crease its thermal conductivity. The theory might possibly be tested by experiments on wires similar material, some of which possessed a more marked crystalline structure (by annealing, r example) than others. The former should then give greater changes in conductivity than the tter.

AUTHOR'S reply (communicated): In reply to Prof. Rankine it may be stated that in all cases ght and left-handed twists were given, and the decreases obtained though of the same order ere not usually quite the same for the two senses. The iron wire was removed and measure-ents made on the portion in which the heat flowed towards the clamped end, instead of from it, nd decreases of the same order were obtained in the two positions.

With regard to the suggestion of Mr. F. E. Smith, the author acknowledges that the experi-ents would be more valuable if performed on single crystals, but would like to point out that luminium was one of the most difficult metals upon which to experiment for the thermal conduc-ivity, which seemed to undergo very erratic changes. Of the other substances which might be sed in the form of single crystals, tin and lead are very soft and therefore not easy to manipulate.

The changes found are much larger than would be expected from the changes in dimensions. rom results of Poynting (Collected Papers, p. 411) for steel and copper, the changes in length nd diameter of the wires used should be of the order of one in a million.

The author is grateful for Dr. Barratt's theory of the results, but it remains a difficult matter to xplain why torsion decreases both conductivities by unequal amounts, while stretching increases ne and decreases the other.

* Proc. Phys. Soc., Vol. 29, p. 195 (1917).

A GENERALISATION OF PROFESSOR WHITEHEAD'S THEORY OF RELATIVITY.

By G. Temple, *B.Sc.*, Birkbeck College, London.

Received October 18, 1923.

Abstract.

This Paper gives the generalisation of Prof. Whitehead's theory of relativity appropriate to the case of a space-time manifold of uniform and isotropic curvature. The general equations of the gravitational and electromagnetic fields are obtained, and these are applied to the discussion of the problems of planetary motion and of the deviation of light rays in the solar field.

CONTENTS.

Introduction.
Classification of Riemannian manifolds.
Congruent Transformations.
The Generalised Theory.
The Retarded Potentials.
Planetary Motion.
Electromagnetic Equations.
Gravitation and Light Waves.
Conclusion.
References.

Introduction.

In the classical theory of relativity expounded by Albert Einstein the paths of material particles and the tracks of rays of light are given as the geodesics and the minimal lines of a four-dimensional manifold, which is specified, partly by a set of differential equations satisfied by the coefficients $g_{\mu\nu}$ in the expression for the linear element

$$ds^2 = g_{\mu\nu} dx_\mu dx_\nu *$$

and partly by the imposition of conditions of simplicity and symmetry, the arbitrary character of which has not escaped criticism at the hands of M. Painleve[1] and the more conservative French mathematicians. Successive generalisations of this theory by Hermann Weyl [2] and Prof. A. S. Eddington[3] have shown that not only the equations of dynamics, but also those of electromagnetism can be regarded as expressing the characteristics of a manifold in which the relation of congruence between infinitesimal tensors is not necessarily transitive. However elegant and interesting may be these mathematical researches, they are open to the objection

* Unless it be otherwise specifically stated, we shall always adopt the usual convention introduced by Einstein that the repetition of a suffix implies summation for all values which that suffix may assume.

urged by Drs. Dorothy Wrinch and Harold Jeffreys[4] relative to the disparity between the principle of the irrelevance of the system of co-ordinates stressed by Eddington and the methods of measurement used in physics, wherein the co-ordinate-system is chosen for its simplicity and propriety to the subject of investigation.

Considerations similar to these lend support to the theory advanced by Dr. Ludvik Silberstein,[5] in which a clear distinction is drawn between the actual space-time manifold and the fictitious dynamic manifold of which the equations of motion give the geodesics. Unfortunately, Silberstein's theory does not lead to the secular advance of the perihelion of Mercury predicted by Einstein, although this perihelionic motion can be accounted for on this theory, as it can also on Newton's, by suitable distributions of unobserved gravitating matter.[6]

In the theory of relativity due to Prof. A. N. Whitehead[7] the linear element of the metrical (space-time) manifold is given in the form

$$dG^2 = c^2dt^2 - dx^2 - dy^2 - dz^2$$

where the time t is supposed to be measured by clocks, and the spatial co-ordinates (x, y, z) by rigid material scales. The path of a material particle of mass M is given as a geodesic of the dynamic manifold whose linear element is

$$dJ^2 = dG_M{}^2 - \frac{2}{c^2}\psi_m \cdot dG_m{}^2,$$

where (1) $dG_M{}^2$ is the linear element along the actual path described by the particle M; (2) $dG_m{}^2$ is the linear element along the actual path described by any other particle of mass m, causally related to M by situation on the same minimal line; (3) and, the retarded potential ψ_m with a moving singularity due to m, expresses the law of the diminishing intensity of the perturbing influence of other particles.

This theory leads to the same results for planetary motion and for the deviation of light rays in the solar field as the theory of Einstein.

A critical summary of the arguments by which Whitehead seeks to establish the above expression for the linear element of the metrical manifold has been given by Prof. C. D. Broad,[8] and a somewhat similar course of reasoning is put forward by Prof. Samuel Alexander.[9] These epistemological arguments, if valid, can only show that the metrical manifold possesses uniform and isotropic curvature; so that we are challenged with the problem of generalising the theory on the basis of a manifold of constant curvature.

Classification of Riemannian Manifolds.

Apart from the philosophical reasons advanced by the writers mentioned above in support of the uniformity of curvature of the metrical manifold, it is possible to show that such a manifold is the simplest generalisation of the flat manifold used in Whitehead's original work, and we proceed to do so the more readily, as we are thereby enabled to indicate the relations between the theories of Whitehead, Einstein and de Sitter.

The standard textbooks on the subject of Riemannian geometry are those of Bianchi and Killing,[10] while the applications to relativistic theories have been examined by G. Herglotz,[11] Schouten and Struik[12] and J. E. Campbell.[13]

The ordered number tetrad (x_1, x_2, x_3, x_4) will be denoted by (x_μ) and will be

called a point. The distance ds between the points (x_μ) and $(x_\mu+dx_\mu)$ is defined by the equation

$$ds^2=g_{\mu\nu}dx_\mu dx_\nu$$

As usual, let g denote the symmetric determinant of the sixteen functions $g_{\mu\nu}$, and $g^{\mu\nu}$ denote the cofactor of $g_{\mu\nu}$ in g divided by g. The Christoffel three-index symbols are defined by the equations

$$\left[\begin{matrix}\mu\nu\\ \sigma\end{matrix}\right]=\tfrac{1}{2}\left(\frac{\partial g_{\mu\sigma}}{\partial x_\nu}+\frac{\partial g_{\nu\sigma}}{\partial x_\mu}-\frac{\partial g_{\mu\nu}}{\partial x_\sigma}\right)$$

$$\left\{\begin{matrix}\mu\nu\\ \sigma\end{matrix}\right\}=g^{\sigma\lambda}\left[\begin{matrix}\mu\nu\\ \lambda\end{matrix}\right]$$

A geodesic is defined by the condition that the integral

$$\int ds$$

taken along the geodesic between two fixed points must be stationary for small variations in the path.

It follows that the equations of a geodesic are

$$\frac{d^2x_\alpha}{ds^2}+\left\{\begin{matrix}\mu\nu\\ \alpha\end{matrix}\right\}\frac{dx_\mu}{ds}\cdot\frac{dx_\nu}{ds}=0,\quad \alpha=1,\ 2,\ 3,\ 4,$$

or, eliminating s,

$$\frac{d^2x_\nu}{dx_1^2}+\left[\left\{\begin{matrix}\lambda\mu\\ \nu\end{matrix}\right\}-\left\{\begin{matrix}\lambda\mu\\ 1\end{matrix}\right\}\frac{dx_\nu}{dx_1}\right]\frac{dx_\lambda}{dx_1}\cdot\frac{dx_\mu}{dx_1}=0,\quad \nu=1,\ 2,\ 3.\ ^{(14)}$$

The Riemannian four-index symbol is defined by the equation

$$B_{\mu\nu\rho\sigma}=\frac{\partial}{\partial x_\nu}\left[\begin{matrix}\mu\rho\\ \sigma\end{matrix}\right]-\frac{\partial}{\partial x_\mu}\left[\begin{matrix}\nu\rho\\ \sigma\end{matrix}\right]-g^{\alpha\beta}\left\{\left[\begin{matrix}\mu\rho\\ \alpha\end{matrix}\right]\left[\begin{matrix}\nu\sigma\\ \beta\end{matrix}\right]-\left[\begin{matrix}\mu\sigma\\ \alpha\end{matrix}\right]\left[\begin{matrix}\nu\rho\\ \beta\end{matrix}\right]\right\}$$

The direction of any line through the point (x_μ) at this point is uniquely specified by the ratios of the differentials (dx_μ) along the line at this point:

$$dx_1 : dx_2 : dx_3 : dx_4=\xi_1 : \xi_2 : \xi_3 : \xi_4$$

One and only one geodesic passes through the point (x_μ) in the direction defined by these three equations.

Let $(\xi^1{}_\mu)$ and $(\xi^2{}_\mu)$ be two sets of quantities defining two directions through the point (x_μ). Then the geodesic surface specified by the elements $(x_\mu\,;\ \xi^1{}_\mu\,;\ \xi^2{}_\mu)$ is defined as the two-dimensional manifold formed by the pencil of geodesics through the point (x_μ) in the directions (ξ_μ) given by the equation

$$\xi_\mu=\alpha\xi^1{}_\mu+\beta\xi^2{}_\nu$$

where α and β are arbitrary parameters.

The Gaussian curvature of this manifold is denoted by (12), and is defined by the equations

$$(12)\quad (\delta_{11}\delta_{22}-\delta_{12}{}^2)=B_{\mu\nu\rho\sigma}\,\xi^1{}_\mu\xi^2{}_\nu\xi^1{}_\rho\xi^{2\sigma}$$

$$\delta_{\alpha\beta}=g_{\mu\nu}\xi_\mu{}^\alpha\xi_\nu{}^\beta$$

Four mutually orthogonal directions through the point (x_μ) are defined by the quantities (ξ_μ^λ) which satisfy the equations

$$g_{\mu\nu}\xi_\mu^\lambda\xi_\nu^\kappa=0 \text{ if } \lambda\neq\kappa$$

at the point (x_μ).

The mean curvature of the four dimensional manifold at the point (x_μ) is defined as the sum of the Gaussian curvatures of the six two-dimensional geodesic manifolds defined by the elements $(x_\mu\,;\,\xi_\mu^\lambda\,;\,\xi_\mu^\kappa)$, where (ξ_μ^λ) and (ξ_μ^κ) are the directions of any pair of any four mutually orthogonal directions through (x_μ) ; and it is given by the equation

$$G=B_{\mu\nu\rho\sigma}\cdot g^{\mu\rho}\cdot g^{\nu\sigma}$$

which shows that it depends only on the point selected, and not on the four mutually orthogonal directions chosen.

The sum of the curvatures of three of these two dimensional manifolds all normal to the direction (ξ_μ^4) is given by

$$(23)+(31)+(12)=\frac{G_{\mu\nu}\cdot\xi_\mu^4\xi_\nu^4}{g_{\mu\nu}\cdot\xi_\mu^4\xi_\nu^4}$$

where

$$G_{\mu\nu}=B_{\mu\sigma\nu\sigma}\cdot g^{\rho\sigma}$$

and is determined completely by the point (x_μ), and the direction (ξ_μ^4). It may be appropriately named the mean curvature at the point (x_μ) normal to the direction (ξ_μ^4).

The following four theorems will now be evident:

(1) (Lipschitz's Theorem): The necessary and sufficient conditions that a manifold should be flat are

$$B_{\mu\nu\rho\sigma}=0 \text{ (twenty independent equations)}$$

ds^2 can then be reduced to the form $dx_1^2+dx_2^2+dx_3^2+dx_4^2$

(2) The necessary and sufficient conditions that the Gaussian curvature should be the same for all points, and for all the geodesic surfaces through a point, are

$$B_{\mu\nu\rho\sigma}=\frac{1}{R^2}(g_{\mu\rho}g_{\nu\sigma}-g_{\mu\tau}g_{\nu\rho})$$

ds^2 can then be reduced to the form

$$\frac{\left(1+\frac{1}{R^2}\Sigma x_\iota^2\right)(\Sigma dx_\iota^2)-\frac{1}{R^2}(\Sigma x_\iota dx_\iota)^2}{\left(1+\frac{1}{R^2}\Sigma x_\iota^2\right)^2},\quad \iota=1,\ 2,\ 3,\ 4$$

(3) The necessary and sufficient conditions that the mean curvature at a point normal to a given direction should be the same for all points and directions through them are

$$G_{\mu\nu}=\frac{3}{R^2}g_{\mu\nu} \text{ (six independent equations)}$$

(4) The necessary and sufficient condition for the uniformity of the mean curvature at all points is

$$G=\frac{12}{R^2}$$

In all the formulæ given above, R may be called the constant (radius) of curvature. It has the dimensions of a distance.

The last equation is satisfied by the coefficients $g_{\mu\nu}$ of the dynamic manifold in all theories of relativity including Einstein's comological theory[15] in which the line element has the form

$$ds^2=c^2\,.\,dt^2-dr^2-R_1{}^2\sin^2\frac{r}{R_1}\,(d\varphi^2+\sin^2\varphi\,.\,d\theta^2) \text{ where } 2R_1{}^2=R^2$$

The manifolds used by Silberstein and Eddington[16] are determined by equation (3), and the line-element has the form

$$ds^2=\gamma\,.\,dt^2-\gamma^{-1}\,.\,dr^2-r^2(d\varphi^2+\sin^2\varphi\,.\,d\theta^2)$$

where

$$\gamma=1-\frac{2m}{r}-\frac{r^2}{R^2}$$

The earlier theory of Einstein and that of Whitehead lead to dynamic manifolds for which

$$G_{\mu\nu}=0$$

The manifold of equation (2) is that used by de Sitter,[17] and is the extension to four dimensions of the type of manifold to which we are led by the investigations of Marius Sophus Lie [18] in his classical solution of the Riemann-Helmholtz problem.

The geodesics of such a manifold when the linear element has the form quoted above are given by three linear equations in (x_1, x_2, x_3, x_4). Defining the interval between the points (u_i') and (u_i'') as the integral

$$S=\int ds$$

taken along the geodesic joining these two points, we find that

$$(1)\quad \sin^2\left(\frac{S}{R}\right)=\frac{\Sigma(u_i'-u_i'')^2+\frac{1}{R^2}\,\Sigma(u_i'u_j''-u_j'u_i'')^2}{R^2\left(1+\frac{1}{R^2}\,\Sigma u_i'^2\right)\quad\left(1+\frac{1}{R^2}\,\Sigma u_i''^2\right)},\quad i,j=1,\ 2,\ 3,\ 4;$$

(2) S=logarithm of the anharmonic ratio of the points (u_ι') and (u_ι'') and the points in which the geodesic intersects the Absolute

$$1+\frac{1}{R^2}\,\Sigma x_\iota{}^2=0^{(19)},\ \iota=1,\ 2,\ 3,\ 4;$$

(3) In particular, the condition that the points (u_μ') and (u_μ'') should be on the same minimal-line, or "co-null" is

$$\Sigma(u_\mu'-u_\mu'')^2+\frac{1}{R^2}\,\Sigma(u_\mu'u_\nu''-u_\mu''u_\nu')^2=0,\ \mu,\ \nu=1,\ 2,\ 3,\ 4.$$

Congruent Transformations.

The congruent transformations of a manifold of uniform and isotropic curvature in which the interval S between the points (u_ι) and (v_ι) is given by

$$\cos\left(\frac{S}{R}\right)=\frac{(u,v)}{\sqrt{(u,u)\,.\,(v,v)}}$$

where
$$(\alpha,\beta)=1+\frac{1}{R^2}\Sigma\alpha_\iota\beta_\iota \quad , \quad \iota=1,\ 2,\ 3,\ 4$$

are the homographic transformations admitted by the equation to the Absolute

$$(x,\ x)=0,$$

and the infinitesimal operators of this ten-parameter group are

$$x_j\frac{\partial}{\partial x_i}-x_i\frac{\partial}{\partial x_j} \qquad i,\ j=1,\ 2,\ 3,\ 4 \text{ (“ rotations ”)}$$

and
$$\frac{\partial}{\partial x_i}+\frac{1}{R^2}x_i\Sigma x_j\frac{\partial}{\partial x_j} \qquad i,\ j=1,\ 2,\ 3,\ 4 \text{ (“ translations ”).}$$

We shall afterwards have occasion to solve a differential equation in

$$V(x_1,\ x_2,\ x_3,\ x_4)$$

which is cogrediently transformed with the equation to the absolute. We notice that if

$$V=\varphi\left\{\frac{1}{\sqrt{(u,\ u)}}\right\}$$

is a solution of this equation, so also is

$$V=\varphi\left\{\frac{(u,\ v)}{\sqrt{(u,\ u)\,.\,(v,\ v)}}\right\}$$

where (v_i) are any four arbitrary constants

The Lorentz Transformation.

The sub-group of the congruence group, which leaves invariant the origin $(x_i=0)$, is evidently the general quaternary orthogonal group. If the plane $x_3=x_4=0$ is also self-corresponding, the equations of transformation have the canonical form

$$x_1{}^1=\frac{1-\lambda^2}{1+\lambda^2}x_1+\frac{2\lambda}{1+\lambda^2}x_2$$

$$x_2{}^1=\frac{-2\lambda}{1+\lambda^2}x_1+\frac{1-\lambda^2}{1+\lambda^2}x_2$$

and are admitted by the equation

$$R^2+x_1{}^2+x_2{}^2=0.$$

To deduce the Lorentz transformation, write

$$x_1=x, \quad x_2=ict, \qquad i^2=-1$$

$$\lambda=\tan\frac{\varphi}{2}, \qquad \frac{v}{c}=\tan\varphi.$$

Then

$$x^1=\beta(x-\lambda t)$$

$$t^1=\beta\left(t-\frac{vx}{c^2}\right)$$

where

$$\beta^2=\left(1-\frac{v^2}{c^2}\right)$$

and c is an invariant velocity.

From these formulæ it is easy to deduce the formula for the composition of velocities

$$V^1=\frac{V-U}{1-\frac{UV}{c^2}}$$

where $$V=\frac{dx}{dt}, \quad V^1=\frac{dx^1}{dt^1}, \quad U=v.$$

We proceed to show directly that this formula is the simplest generalisation of the Newtonian law,

$$V^1=V-U.$$

Let A, B, C be three particles moving along the same straight line, and let u, v, w be the velocities of C, A, B relative to B, C, A respectively.

Assume that $$f(u, v, w)=0.$$

Introduce three variables x, y, z, known as the "rapidities" of the particles, whose velocities are u, v, w,[20] and subjected to the conditions

$$u=\varphi(x), \; v=\varphi(y), \; w=\varphi(z),$$
$$x+y+z=0.$$

Since $f(u, v, w)$ must be symmetrical in u, v, w, if $\varphi(x)$ is continuous, it is restricted to be a Weierstrassian function of x [21]—i.e., an algebraical function of

(1) x,

or (2) $\varepsilon^{\pi ix}$(a singly periodic function),

or (3) $p(x)$—Weierstass's elliptic function (a doubly periodic function).

Further restrictions on the form of $\varphi(\ )$ may be imposed by the following physical considerations:—

(1) If $u=v=w=0$, $x=y=z=0$ $\quad\therefore \varphi(0)=0.$

(2) The equation $u=\varphi(x)$ must have only one root and that real.

We are now left with the following possibilities :—

(1) $\varphi(x)$ is an algebraical function of the first degree—i.e., $\varphi(x)=\lambda x$.

This leads to the Newtonian law

$$u+v+w=0.$$

(2) $\varphi(x)$ is an odd function with an imaginary period. The simples case is $\varphi(x)=c\,.\tanh x$, which leads to

$$u+v+w+\frac{uvw}{c^2}=0.$$

(3) $\varphi(x)$ is an elliptic function with both periods complex.

Hence the second case gives the simplest generalisation of the Newtonian law. Writing $u=-U$, $v=V$, $w=-V^1$, we regain the equation

$$V^1=\frac{V-U}{1-\frac{UV}{c^2}}$$

The Generalised Theory.

By the fundamental principle of relativity, the transformations to which the equations expressing physical laws are subjected when the linear element of the metrical manifold is subjected to a congruent transformation, must form a group isomorphic with the congruence group. The two simplest methods of ensuring the isomorphism of the two groups is to take the group of transformations of the physical laws as cogredient or contragredient to the congruence group.[22]

Let $ds^2=g^{(x)}_{\mu\nu}dx_\mu dx_\nu$

be the linear dement of the metrical manifold, and

$$x_\mu=f_\mu(u) \qquad \mu=1,\ 2,\ 3,\ 4$$

a transformation of the congruence group from variables (x_μ) to (u_μ).

Since ds^2 is an invariant

$$g^{(u)}_{\mu\nu}=g^{(x)}_{\alpha\beta}\frac{\partial x_\alpha}{\partial x_\mu}\cdot\frac{\partial x_\beta}{\partial x_\nu}$$

In order to obtain a tensor $T_{\mu\nu}$ transformed cogrediently with $g_{\mu\nu}$ we may take

$$T_{\mu\nu}=\frac{\partial T}{\partial x_\nu}-\frac{\partial T_\nu}{\partial x_\mu}. \qquad \ldots\ldots\ldots\ldots \quad (1)$$

where the tensors (T_μ) and (dx_μ) are contragredient

or

$$T_{\mu\nu}=T_{\mu(\nu)}. \qquad \ldots\ldots\ldots\ldots \quad (2)$$

where the bracketed suffix denotes covariant differentiation with respect to the variable x_ν, according to the definition

$$T_{\mu(\nu)}=\frac{\partial T_\mu}{\partial x}-\left\{\begin{matrix}\mu\nu\\ \lambda\end{matrix}\right\}T_\lambda$$

To obtain a tensor contragredient to (x_μ) we may take

$$T_\mu=\frac{\partial I}{\partial x_\mu}$$

where I is an invariant.

It is convenient to note here that if $A^{\alpha\beta}$ is a second order contravariant tensor

then $$A^{\alpha\beta}{}_{(\sigma)}=\frac{\partial A^{\alpha\beta}}{\partial x_\sigma}+\left\{\begin{matrix}\sigma\mu\\ \alpha\end{matrix}\right\}.A^{\mu\beta}+\left\{\begin{matrix}\sigma\mu\\ \beta\end{matrix}\right\}.A^{\alpha\mu}$$

is a mixed third order tensor (see below under Electromagnetic Equations).

We are now in a position to give the appropriate generalisation of Whitehead's equations. We take as the equation defining the potential that used by Silberstein,

$$g^{\mu\nu}.V_{\mu(\nu)}=0$$

The potential mass impetus dJ and the potential electro-magnetic impetus dF of a kinematic element pervaded by a particle of mass M and charge E are defined by the equations

$$dJ^2=dG^2{}_{\mathrm{M}}-\frac{2}{C^2}\Sigma.V_m.dG_m{}^2$$

$$dF=\sum_\mu F_\mu.dx_\mu,\ \mu=1,\ 2,\ 3,\ 4,$$

where the terms of the first equation have been defined in the Introduction, and those of the second will be defined in the section dealing with Electromagnetic Equations.

The path of the particle is to be such as to render the integral

$$\int dI,\quad dI=MdJ+c^{-1}E.dF$$

taken along the path a minimum.

This condition leads to the equations

$$\frac{d}{dx_4}\left\{\frac{M}{\Gamma}.\frac{\partial}{\partial\dot{x}_\mu}\left(-\frac{1}{2}c^2\Gamma^2\right)\right\}=\frac{M}{\Gamma}\frac{\partial}{\partial x_v}\left(-\frac{1}{2}c^2\Gamma^2\right)+.E.\Sigma.F_{\mu\rho}.x_\rho$$

where $$c^2\Gamma^2=\frac{dJ^2}{dx_4{}^2}$$

and $$F_{\mu\rho}=\frac{\partial F_\mu}{\partial x_\rho}-\frac{\partial F_\rho}{\partial x_\mu}$$

The Retarded Potentials.

We proceed to obtain a solution of the potential equation

$$g^{\mu\nu}.V_{\mu(\nu)}=0$$

appropriate to the case of a point singularity, moving with a velocity less than c, which will be shown to be the velocity of light in the absence of a gravitational field.

Transforming from the geodetic (x_i) to spherical polar co-ordinates (v_i) by means of the equations

$$\begin{aligned}x_1&=R\tan v_1.\sin v_2.\sin v_3.\sin v_4\\ x_2&=R\tan v_1.\sin v_2.\sin v_3.\cos v_4\\ x_3&=R\tan v_1.\sin v_2.\cos v_3\\ x_4&=R\tan v_1.\cos v_2\end{aligned}$$

we obtain as the expression of the line element

$$ds^2=R^2\{dv_1{}^2+\sin^2v_1[dv_2{}^2+\sin^2v_2\,(dv_3{}^2+\sin^2v_3\,dv_4{}^2)]\}$$

The solution of the potential equation expressed in this system of co-ordinates, bject to the conditions

(1) V is a function of v_1, only

(2) $\underset{R\to\infty}{Lt}\,.\,V=\dfrac{1}{R^2{v_1}^2}$

$$V=\frac{1}{R^2}\operatorname{cosec} v_1\,.\,\cot v_1-\frac{1}{R^2}\log\tan\frac{v_1}{2}$$

$$=f(v_1)$$

Now the interval S between event-particles (o, o, o, o) and (x_1, x_2, x_3, x_4) is given by

$$\cos\left(\frac{S}{R}\right)=(x,x)^{-\frac{1}{2}}$$

hence

$$Rv_1=S.$$

Hence by the theorem in the section on Congruent Transformations, since $=f\{\cos^{-1}(x, x)^{-\frac{1}{2}}\}$ is a solution of the potential equation, so also is

$$V=\frac{1}{R^2}\,.\,\frac{\sigma}{1-\sigma^2}-\frac{1}{2R^2}\log\frac{1-\sigma}{1+\sigma}$$

here

$$\sigma=\frac{(x,\,p)}{\sqrt{(x,x)\,,\,(p,p)}}$$

nd (p_1, p_2, p_3, p_4) are arbitrary constants.

We now use Prof. Conway's method[23] to obtain the solution for a moving ngularity.

Lemma.—(1) The transformation from geodetic co-ordinates (x_i) to the actual patial-temporal co-ordinates (x, y, z, t) is $x_1=x$, $x_2=y$, $x_3=z$, $x_4=ict$, where $i^2=-1$.

(2) Let $p_i=p_i(\lambda)$ be the parametric equations to the path of the moving singu-rity in geodetic co-ordinates, and $x=\xi(t)$, $y=\eta(t)$, $z=\zeta(t)$ in actual spatial-temporal o-ordinates.

Then, by Liénard's Theorem, [24] provided that

$$\left(\frac{\partial\xi}{\partial t}\right)^2+\left(\frac{\partial\eta}{\partial t}\right)^2+\left(\frac{\partial\zeta}{\partial t}\right)^2<c^2$$

e path of the moving singularity intersects the minimal surface

$$x^2+y^2+z^2=c^2t^2$$

one, and only one, real point, such that $p_4<o$

Hence the equation in λ

$$\Sigma p_i^{\,2}(\lambda)=0$$

as only one real negative root.

By means of a congruent transformation, move the origin to (x_1, x_2, x_3, x_4). Then we see that the equation in λ

$$\Sigma[x_i-p_i(\lambda)]^2+\frac{1}{R^2}\Sigma[x_ip_j(\lambda)-x_j\,.\,p_i(\lambda)]^2=0$$

has only one real root such that $p_4 < x_4$.

Since the poles of

$$f(\lambda)=\frac{1}{R^2}\,.\,\frac{\sigma}{1-\sigma^2}-\frac{1}{R^2}\log\frac{1-\sigma}{1+\sigma}$$

are determined by the above equation, $f(\lambda)$ has one, and only one, pole on the real axis, say, $\lambda=\Lambda$

The contour integral

$$V=\frac{1}{2\pi i}\int_C f(\lambda)\,.\,g(\lambda)\,.\,d\lambda$$

where C, the path of integration in the plane of the complex variable λ includes the one pole of $f(\lambda)$ on the real axis, and includes no pole of $g(\lambda)$, will be a solution of the potential function.

Evaluating the integral, we have $V=-\frac{g(\lambda)}{R^2}\,.\,\frac{d\lambda}{d\sigma}$

Now,
$$\sigma=\frac{(x,\,p)}{\sqrt{(x,\,x)\,.\,(p,\,p)}}$$

and
$$ds=\frac{\sqrt{(p,p)\Sigma\dot{p}_i^2-\frac{1}{R^2}(\Sigma p_i\dot{p}_i)^2}}{(p,\,p)}d\lambda=\frac{\Omega^{-1}\,d\lambda}{(p,\,p)},\ \text{say,}$$

where
$$\dot{p}_i=\frac{dp_i}{d\lambda}$$

are invariants with respect to the congruence group.

Hence $\frac{d\sigma}{d\lambda}\,.\,d\lambda$ and $\Omega\,\frac{d\sigma}{d\lambda}\,.\,(p,\,p)$ are invariants.

Writing $g(\lambda)=-\Omega^{-1}\,.\,(p,\,p)^{-1}$, the expression for V becomes invariant, and equals

$$\frac{(x,\,x)^{\frac{1}{2}}\,.\,(p,\,p)^{\frac{1}{2}}\,.\,\Omega^{-1}}{\Sigma\,(x_i-p_i)\dot{p}_i+\frac{1}{R^2}\Sigma x_ip_j(\dot{p}_ip_j-p_i\dot{p}_j)}\qquad i,j=1,\,2,\,3,\,4$$

where the event-particles (x_i) and (p_i) are co-null,

i.e., $$(x,\,x)\,.\,(p,\,p)=(x,\,p)^2$$

If the point-singularity is at rest at the origin,

$$p_1=p_2=p_3=0$$

and
$$V=\frac{1+\frac{x_4p_4}{R^2}}{x_4-p_4}$$

Planetary Motion.

The differential form

$$ds^2=R^2(dv_1^2+\sin^2 v_1 \,.\, dv_2^2)$$

mits the substitution of period two

$$\cos v_1=\sin \omega_1 \,.\, \sin \omega_2 \quad ; \quad \cos \omega_1=\sin v_1 \,.\, \sin v_2$$

$$\cot v_2=\tan \omega_1 \,.\, \cos \omega_2 \quad ; \quad \cot \omega_2=\tan v_1 \,.\, \cos v_2$$

that the linear element of the metric manifold becomes

$$ds^2=R^2\{d\omega_1^2+\sin^2\omega_1 \,.\, d\omega_2^2+\cos^2\omega_1(d\omega_3^2+\sin^2\omega_3 \,.\, d\omega_4^2)\}$$

$$\omega_4=v_4=\varphi, \quad \omega_3=v_3=\theta.$$

riting

$$\frac{r}{R}+\omega_1=\frac{\pi}{2}, \quad \omega_2=\frac{ict}{R}, \quad dG^2+ds^2=0,$$

have

$$dG^2=c^2\cos^2\frac{r}{R} \,.\, dt^2-dr^2-R^2\sin^2\frac{r}{R}(d\theta^2+\sin^2\theta \,.\, d\varphi^2)$$

We note (1) that r is the interval between event-particles

$$(0, 0, 0, p_4) \text{ and } (x_1, x_2, x_3, x_4)$$

d that therefore

$$V=\frac{1}{R}\cot\frac{r}{R}$$

a solution of the potential equation for a point singularity stationary at the origin;

that

$$\frac{x_4}{R}=\cot\frac{ict}{R}$$

t,

$$\frac{p_4}{R}=\cot\frac{icp}{R}$$

or motion in the plane of a planetary orbit (φ—const)

$$dG^2=c^2\cos^2\frac{r}{R} \,.\, dt^2-dr^2-R^2 \,.\, \sin^2\frac{r}{R} \,.\, d\theta^2$$

ne condition for causal correlation may be written in Cayley's notation,

$$\frac{r}{R}=gd \,.\, \frac{c}{R}(t-p), \quad \text{i.e., } \tan\frac{r}{R}=\sinh\frac{c}{R}(t-p)$$

$$(c \,.\, dt-c \,.\, dp)=\sec\frac{r}{R} \,.\, dr$$

In C.G.S. units

$$V=\frac{\gamma m}{R}\cot\frac{r}{R}$$

here m is the solar mass and γ the constant of gravitation.

$$dJ^2=(c^2-2\psi)\cos^2\frac{r}{R} \,.\, dt^2-\left(1+\frac{2}{c^2}\psi\right)dr^2-R^2\sin^2\frac{r}{R} \,.\, d\theta^2$$

$$+\frac{4\gamma m}{cR}\operatorname{cosec}\frac{r}{R} \,.\, dr \,.\, dt$$

here

$$\psi=\frac{2\gamma m}{R}\cot\frac{2r}{R}$$

The expression for the linear element of the dynamic manifold given in a Paper by Silberstein[16] is

$$ds^2=\left(1-\frac{2a}{R}\cot\frac{r}{R}\right)\cos^2\frac{r}{R}\,.\,c^2\,.\,dt^2-\left(1+\frac{2a}{R}\cot\frac{r}{R}\right)dr^2-R^2\sin^2\frac{r}{R}d\theta^2$$

where $$a=\frac{\gamma m}{c^2}$$

This gives for the perihelionic advance in radians per revolution

$$\frac{6\pi}{1-e^2}\left(\frac{2\pi a}{cT}\right)^2+\frac{3\pi}{4}\left(1-\frac{4}{3}e^2+\frac{5}{8}e^4\right)\left(\frac{cT}{\pi R}\right)^2$$

Where a=semi major axis of orbit,
e=eccentricity,
T=periodic time.

Neglecting terms of higher orders than $\frac{1}{c^2}$ or $\frac{c^2}{R^2}$ our expression becomes

if $$\rho=r\left(1-\frac{\gamma m}{c^2 r}\right)$$

$$c^2\Gamma^2=c^2\left(1-\frac{\rho^2}{R^2}\right)-2\psi-\left(1+\frac{2}{c^2}\right)\psi\left(\dot\rho^2+\rho^2\dot\theta^2\right)+\frac{4\gamma m\dot\rho}{c\rho}$$

Hence the equations of motion are

$$\triangle\,\rho^2\dot\theta=\text{(a constant)}-\text{(the new form of Kepler's second law)}$$

$$\frac{d}{dt}(\triangle\,\dot\rho)-\triangle\,\rho\dot\theta^2=\triangle\left\{1+\frac{2K}{c^2}\right\}\frac{d\psi}{d\rho}+\frac{c^2\rho}{\Gamma R^2}$$

where $$\triangle=\frac{1+\frac{2}{c^2}\psi}{\Gamma}\,,\quad K=\frac{1}{2}(\dot\rho^2+\rho^2\dot\theta^2)-\frac{\gamma m}{\rho}$$

Writing $\rho\,.\,u=1$, the equation of the orbit becomes

$$\frac{d^2u}{d\theta^2}+u=\frac{\triangle^2}{h^2}\left(1+\frac{2K}{c^2}\right)\frac{d\psi}{du}-\frac{\triangle^2\,.\,c^2\,.\,u^{-3}}{h^2R^2}$$

whence the perihelionic advance in radians per revolution is

$$\left\{\frac{3\gamma^2m^2}{c^2h^2}+\frac{3c^2h^6}{2R^2\gamma^4m^4}\right\}2\pi$$

This agrees with the expression given by Eddington[25] and derived from th dynamic manifold

$$ds^2=c^2.\gamma.dt^2-\gamma^{-1}.dr^2-r^2d\theta^2$$

where $$\gamma=1-\frac{2\gamma m}{c^2r}-\frac{r^2}{R^2}$$ in our units.

bstituting

$$T=87\cdot97 \text{ days}$$
$$a=5\cdot77\times10^7 \text{ km.}$$
$$c=3\times10^5 \text{ km./sec.}$$
$$e=0\cdot206,$$

e find that in the case of Mercury, the condition that the ratio of the second term the first should be less than 1 per cent. is that

$$R>2\cdot5\times10^{16} \text{ km.}$$

Electromagnetic Equations.

The skew covariant tensor $F_{\mu\nu}$ is defined by the equations

$$F_{\mu4}=ie_\mu$$
$$c\,.\,F_{\lambda\mu}=h_\nu \qquad (\lambda,\ \mu,\ \nu \text{ all unequal}),$$

here e_μ and h_μ are the components of the electric and magnetic forces in geodetic b-ordinates.

Let $J_{\mu\nu}$ be the fundamental tensor $g_{\mu\nu}$ of the dynamic manifold and

$$F^{\mu\nu}=J^{\rho\mu}\,.\,J^{\sigma\nu}\,.\,F_{\rho\sigma}.$$

The contravariant electric motion tensor J^μ is defined in geodetic co-ordinates y the equations

$$J^\mu=\frac{4\pi\rho}{c}\,.\,\dot{x}_\mu$$

where ρ is the electric density and $\dot{x}_\mu=\dfrac{dx_\mu}{dx_4}$

We take as our electromagnetic equations

$$\frac{\partial F_{\mu\nu}}{\partial x_\lambda}+\frac{\partial F_{\nu\lambda}}{\partial x_\mu}+\frac{\partial F_{\lambda\mu}}{\partial x_\nu}=0 \quad (\lambda,\ \mu,\ \nu \text{ all unequal})$$
$$F^{\mu\nu}{}_{(\nu)}=J^\mu$$

The first set of equations is satisfied by writing

$$F_{\mu\nu}=\frac{\partial F_\mu}{\partial x_\nu}-\frac{\partial F_\nu}{\partial x_\mu}$$

and
$$F^\alpha{}_{(\alpha)}=(J^{\mu\alpha}F_\mu)_{(\alpha)}=0$$

These equations correspond to the equations of Lorentz

$$\frac{1}{c}\,.\,\frac{\partial h}{\partial t}+\text{curl } e=0=\text{div } h$$

The second set of equations becomes

$$\sum_\alpha\frac{\partial F^{\mu\alpha}}{\partial x_\alpha}=J^\mu-\sum_\rho F^{\mu\rho}\,.\,\frac{\partial}{\partial x_\rho}\,.\,\log(-J)^{\frac{1}{2}} \text{ (see above under the generalised theory)}$$

where J=the determinant of the $J_{\mu\nu}$'s.

The last term is omitted by Whitehead and included by Eddington. Both

sets of equations thus obtained possess the tensor character, but those given above render the following analysis simpler. These equations correspond to the equations of Lorentz :—

$$-\frac{1}{c}\cdot\frac{de}{dt}+\text{curl } h=\frac{4\pi\rho u}{c}$$

$$\text{div } e=4\pi\rho.$$

We have that

$$J_\mu=J_{\mu\alpha}J^\alpha=(J^{\alpha\beta}F_{\mu\beta})_{(\alpha)}=J^{\alpha\beta}F_{\mu(\beta)(\alpha)}+(JG)^\epsilon_\mu\cdot F_\epsilon$$

where $(JG)^\epsilon_\mu$=same function of $J_{\mu\nu}$ as G^ϵ_μ is of $g_{\mu\nu}$

Following the researches of Hadamard and E. Vessiot[26] we take the light rays as the bicharacteristics of this equation of wave-propagation.

Provided that $(JG)^\epsilon_\mu$ contains only the first derivatives of $J_{\mu\nu}$, the characteristics of the above equation are the surfaces

$$\varphi(x_1, x_2, x_3, x_4)=0$$

where

$$J^{\sigma\tau}\frac{\partial\varphi}{\partial x_\sigma}\cdot\frac{\partial\varphi}{\partial x_\tau}=0$$

and the bicharacteristics are the solutions of

$$J_{\sigma\tau}\frac{dx_\sigma}{ds}\cdot\frac{dx_\tau}{ds}=0$$

i.e., the minimal lines $dJ=0$ of the dynamic manifold.

To our degree of approximation, the expression for the linear element of the dynamic manifold is identical with that obtained by Einstein as a solution of the equation

$$G_{\mu\nu}=\frac{3}{R^2}\cdot g_{\mu\nu}$$

Hence we may take $(JG)_\mu{}^\epsilon$ as $J^{\nu\epsilon}\cdot(JG)_{\mu\nu}=\frac{3}{R}J_\mu{}^\epsilon$, which contains no derivatives of $J_{\mu\nu}$; and the preceding analysis will apply. Also, since the gravitational field of the sun is permanent, i.e., $\frac{\partial J_{\mu\nu}}{\partial x_4}=0$, we may follow the analysis of Th. de Donder[27] and apply Huyghen's principle to determine the light tracks.

Let V=velocity of light. Confining ourselves to the region of space within the earth's orbit, we see that

$$\rho<1{\cdot}5\times10^8\text{km., while } R>10^{16}\text{km.}$$

Hence in this region $\frac{\rho}{R}<1{\cdot}5\times10^8$ and we may use the same approximate expression for the linear element of the dynamic manifold; as in the discussion of planetary motion, whence we find

$$V=c\left\{1-\frac{\rho^2}{2R^2}-\frac{2\psi}{c^2}\right\}$$

The paths of the light rays will therefore be the same as in a medium of refractive dex

$$\mu=\left(1+\frac{\rho^2}{2R^2}+\frac{2\gamma m}{c^2\rho}\right)$$

ıe minimum value of the last term is about 2×10^{-8} and the maximum value of ıe middle term about 10^{-16}.

Hence to our degree of approximation the curvature of space has quite an appreciable effect (about 2×10^{-8} of the Einsteinian expression) on the deviation rays in the solar field.

Conclusion.

To summarise the above discussion of the small corrections to Whitehead's ıeory necessitated by assuming the spatial-temporal manifold to be of uniform ınd isotropic curvature, we may say :—

(1) That the observations of the centennial advance of the perihelion of Mercury permit us to assign a lower limit to the space-constant $R-10^{16}$km., which ; considerably higher than the value 8×10^{13}km., obtained from the classical mparison of the values of the parallax of α Centauri determined by the direct ınd Bessel's method[28], and somewhat lower than the value quoted by Eddington -10^{18} km.—as the distance of the furthest globular clusters.

(2) While the effects of the curvature on the value of the angular deviation of ght rays grazing the sun's disc are of the order of 10^{-8} of the whole deviation, and herefore wholly inappreciable.

References.

(1) M. Painleve, " La Mécanique classique et la theorie de la relativité," Comptes Rendus, 1921, pp. 677-680; " La Gravitation dans la Mécanique de Newton et dans la Mécanique d'Einstein," Comptes Rendus, 1921, pp. 873-887.

(2) Hermann Weyl, " Raum-Zeit-Materie."

(3) Prof. A. S. Eddington, " A Generalisation of Weyl's Theory of the Electromagnetic and Gravitational Fields," Proc. Roy. Soc. A., Vol. 99, 1921, pp. 104-122.

(4) Drs. Dorothy Wrinch and Harold Jeffreys, " The Relation between Geometry and Einstein's Theory of Gravitation," Nature, 1921, pp. 806-809.

(5) Dr. Ludvik Silberstein, " General Relativity without the Equivalence Hypothesis," Phil. Mag., Vol. 36, 1918, p. 97.

(6) Dr. H. Jeffreys, Monthly Notices of the Royal Astronomical Society, Vol. 77, 1917, p. 112.

(7) Prof. A. N. Whitehead, " The Principles of Natural Knowledge," " The Concept of Nature," " The Principle of Relativity," 1922 (all Camb. Univ. Press).

(8) Prof. C. D. Broad, " Critical Notices," Mind, Vol. 32, 1923, pp. 211-219.

(9) Prof. Samuel Alexander, " Space, Time and Deity," 1920.

(10) Bianchi, " Lezioni sulla Geometria differenziale," 1903 (Spoerzi, Pisa); W. Killing, " Die Nicht-Euklidischen Raumformen," 1888 (Leipzig).

(11) G. Herglotz, "Zur Einsteinschen Gravitationstheorie," Leipziger Berichti, 1916, p. 199.
(12) J. A. Schouten and D. J. Struik, "On Some Properties of General Manifolds Relating to Einstein's Theory of Gravitation," Am. Jour. Maths., 1921, p. 213.
(13) J. E. Campbell, "Einstein's Theory of Gravitation as an Hypothesis in Differential Geometry," Proc. Lond. Math. Soc., 1922, p. 1.
(14) W. Van Der Woude, "On the Light Path in the General Theory of Relativity," Koninklïgke Akademie van Wetenschnappen te Amsterdam, Proceedings 1923, p. 288.
(15) Albert Einstein, "Kosmologische Betrachtungen zur allgemeinen Relativitätstheorie," Sitz. Berlin, 1917, p. 142; and 1918, p. 270.
(16) Dr. Ludvik Silberstein, "Planetary Motion in Spare-time of Constant Curvature according to the Generalised Principle of Relativity," Monthly Notices of the Royal Society, Vol. 78, 1918, p. 363. Prof. A. S. Eddington, "The Mathematical Theory of Relativity," Camb. Univ. Press, 1923.
(17) De Sitter, "On Einstein's Theory of Relativity and its Astronomical Consequences," III, Monthly Notices of the Royal Society, Vol. 78, 1918, p. 3.
(18) Marius Sophus Lie, "Transformations-gruppen," Band III, p. 471-498, §6, B.G. Teubner, Leipzig.
(19) A. Cayley, "A Sixth Memoir on Quantics," Trans. Roy. Soc., 1859; Coll. Papers, Vol. II. F. Klein, Math. Ann., Vol. IV, 1871.
(20) Dr. A. A. Robb, "A Theory of Time and Space," 1914, par. 98.
(21) Dr. A. R. Forsyth, "Theory of Functions of a Complex Variable," Chap. 13, 1900. Phragmén, Acta Mathematica 7, 1885, pp. 33-42.
(22) M. M. G. Ricci and T. Levi-Civita, "Méthodes de calcul différentiel absolu," Mathematische Annalen, Band 54, 1901, pp. 125-201.
(23) Prof. A. W. Conway, "The Field of Force due to a Moving Electron," Proc. Lond. Math. Soc., Ser. 2, Vol. 1, 1903.
(24) Liénard, L'Éclairage Électrique, Vol. 16, 1898, pp. 5, 53, 106.
(25) Prof. A. S. Eddington, "The Mathematical Theory of Relativity," p.
(26) Hadamard, "Lecons sur la propagation des ondes," 1903, p. 292. E. Vessiot, "Sur la propagation des ondes et sur la theorie de la relativité restreinte," Comptes Rendus, 1921, pp. 349-351.
(27) Th. de Donder, "La Gravifique einsteinienne," Gauthier-Villars, Paris, 1921, p. 40.
(28) Dr. D. M. Y. Sommerville, "The Elements of Non-Euclidian Geometry" (G. Bell & Sons, 1914), pp. 203-220.

DISCUSSION.

Prof. A. N. WHITEHEAD congratulated the Society on publishing the first Paper on this subject from the pen of a young scientist whose work augurs a very distinguished career. The mathematics in the Paper was handled in a way that showed the author to be the master and not the slave of his symbols.

Prof. Whitehead then proceeded to indicate some respects in which his own presentation of Relativity differs from that of what may be called the orthodox school. The latter recognises only one field of natural relations, that is to say a field which is wholly contingent. Thus, to

pecify an interval on Einstein's theory it is necessary to know the distribution of matter through-ut the universe, since the nature of the space-time continuum at any point-event is conditioned by ie gravitational effect of every existing particle, however distant. Prof. Whitehead, on the ther hand, puts forward a more conservative view—and in this he is supported on philosophical rounds by Prof. Alexander—and postulates two fields of natural relations, one of these (viz., pace and time relations) being isotropic, universally uniform, and not conditioned by physical ircumstances ; the other comprising the physical relations expressed by laws of nature, which are dmittedly contingent. Thus by considering the intrinsic properties of an adequate part of the niverse it is possible, on the Whitehead theory, to determine that space-time is isotropic through-ut that part, and to infer that it is isotropic throughout the whole ; but on the Einstein theory here is a serious difficulty in defining any units of measurement in the absence of complete nowledge of the structure of the whole universe, and consequently there is no definite basis for he expression of physical laws, and no means of determining completely the nature of the con-inuum (i.e., the $g_{\mu\nu}$) throughout even a finite part of it.

A further advantage of distinguishing between space-time relations as universally valid and physical relations as contingent is that a wider choice of possible laws of nature (e.g., of gravity) hereby becomes available, and while the one actual law of gravity must ultimately be selected rom these by experiment, it is advantageous to choose that outlook on Nature which gives the reater freedom to experimental inquiry. Of the various laws consistent with this outlook Einstein's is one, but its theoretical aspect is different according as it is regarded from the point f view of one or the other of the two postulates under consideration.

The aim of the Einstein theory is to derive physical laws of such a general character as to e independent of the peculiarities imposed on measurements by the particular circumstances n which they are made. But this is an arbitrary demand : it may well be that those peculiarities re expressions of the essential realities underlying the phenomena. To take a parallel case : f we are interested in a particular ellipse, we may arrive best at some of its properties by thinking f it simply as a curve, at others by thinking of it as a conic, at others by thinking of it as an llipse ; but we shall arrive at other properties of great practical value by considering its par-icular dimensions and position. Einstein's view is analogous to regarding the first-mentioned roperties as objectively real while regarding the last-mentioned properties as accidents arising out of the point of view of the observer. In investigating the laws of nature what really concerns us is our own experiences and the uniformities which they exhibit, and the extreme generalizations of the Einstein method are only of value in so far as they suggest lines along which these ex-periences may be investigated. There is a danger in taking such generalizations as our essential realities, and in particular the metaphorical " warp " in space-time is liable to cramp the imagination of the physicist, by turning physics into geometry. The case is a little like that of a metaphor used by Dr. Routh in lecturing on metacentres. He would draw a ship on his hand in chalk and rock it, and then explain that " you now remove the sea." This illuminating figure of speech if applied too literally to the particular circumstances to which the naval con-structor is limited would give rise to practical difficulties. In short, it seems undesirable to make (as Einstein does) extreme generality—the being independent of all particular conditions of measurement—the criterion by which the real character of the physical field is to be deter-mined.

Mr. T. Smith asked whether the curvature of 4-dimensional space-time implied a limit to the extension of space in the ordinary sense.

The Author, in reply, said that in the same way that in a sphere, in ordinary geometry, the area of the surface and the length of the great circles, or geodesics, are limited, so in curved four-dimensional space-time the volume of three-dimensional sections and the length of the geodesics (viz., the paths of the event-particles) are limited, provided that R is real. If R is imaginary, the manifold is unlimited.

X.—ON THE STRUCTURE OF THE ATOMIC NUCLEUS AND THE MECHANISM OF ITS DISINTEGRATION.

By Dr. Hans Pettersson, Göteborgs Högskola, Sweden.

Received November 8, 1923.

(*Communicated by Prof.* A. W. Porter, *F.R.S.*)

Abstract.

In interpreting the results of their experiments on the disintegration of certain light atoms by the impact of α-particles, Rutherford and Chadwick have supposed that the α-particle transfers its momentum by direct impact to a proton which is a satellite to the rest of the nucleus. The Paper gives arguments for an alternative hypothesis which assumes that the α-particle communicates its energy to the nucleus as a whole, precipitating an explosion which is supposed to have only a limited stability in the case of each of the elements.

In order to interpret the results of their experiments on the artificial disintegration of some of the light elements Sir Ernest Rutherford and Dr. J. Chadwick have propounded what might be called the satellite hypothesis for the mechanism of disintegration.* The H-particle expelled by a swift α-particle from, say, an aluminium atom is assumed to revolve round the nucleus as a kind of satellite. The condition for disintegration would be, that a direct impact should occur between this satellite and the α-particle, giving a momentum to the former sufficiently large to overcome the forces attaching it to the nucleus. The main arguments in favour of this view may, perhaps, be summed up in the following manner.

1. As disintegrability was found to be rather an exceptional quality, limited to a small number of elements having an atomic mass given by $A=4n+3$ (with the sole exception of nitrogen for which $A=4n+2$), it would seem reasonable to attribute this quality to a peculiar structure of their nuclei, viz., that the H-particle expelled from them occupies an exposed position, accessible to a direct "satellite hit" from the α-particle. Other atoms might be assumed either to have no such satellite or a satellite so closely bound to the nucleus that the α-particle fails to give it an impulse of the required intensity.

2. The ratio n/Q between the number of H-particles observed, n, and the total number of α-particles required to produce them, Q, was found to be only a small fraction, about 1/20, of the corresponding ratio h/Q for the number h of swift "natural" H-particles set in motion in hydrogen. This is readily explained by the satellite hypothesis, as the probability of an effective satellite hit must necessarily be only a small fraction of the probability of a "nucleus hit."

* Phil. Mag., Vol. 42, p. 809 (1921); also Phil. Mag., Vol. 44, p. 417 (1922).

3. The velocity of the expelled *H*-particle was found to vary in proportion to that of the impinging α-particle, which would indicate a direct transfer of momentum from the latter to the former, just as the satellite hypothesis requires.

The first of these three arguments loses considerably in importance when one considers that, owing to experimental difficulties, the authors could in general observe only such *H*-particles out of those expelled as had a range superior to that of the natural *H*-particles, i.e., superior to some 30 cm. of air. With two of the elements only, viz., carbon as dioxide and oxygen, was it possible to carry out the tests for *H*-particles down to a range of 9 cm.—with negative results—and in the case of later experiments these tests cannot have been very rigorous.* With a couple of the other elements, silicon and chlorine, particles of a range exceeding 9 cm. were looked for in vain but as for the majority of the elements investigated, disintegration giving *H*-particles of less range than 30 cm. of air would have passed unobserved at these experiments.

By means of new methods, where the chief source of error, namely the "natural" *H*-particles from hydrogen contaminations, has been practically eliminated, the author and Dr. Kirsch, working in the Institut für Radiumforschung at Vienna, have been able to test disintegrating elements for *H*-particles of a range barely exceeding that of the impinging α-particles, i.e., about 8 cm. of air.† Of the four elements hitherto examined by this method three, viz., beryllium, magnesium and boron have been found to undergo disintegration, giving off *H*-particles of the ranges 14, 13 and 12 cm. of air respectively. The fourth element, lithium gave, although in comparatively small number, particles of the range 10 cm.

The results from our own experiments as well as from those of Rutherford and Chadwick are embodied in Table I., where the second column gives the maximal ranges of the *H*-particles expelled in the forward direction. Bearing in mind that of the elements not comprised in the table only one, chlorine, has been tested for *H*-particles of less than 30 cm. range, while the great majority have not been tested at all, it hardly seems correct, as far as the present results go, to say that disintegration is an exceptional quality. Regarding the second point raised in favour of the satellite hypothesis, it must be remembered that the same arbitrary limit imposed by the experimental conditions to the range of the particles observed also applies to the values found for n Q. Now the shape of the absorption curves given by Rutherford and Chadwick for these particles indicates that considerably larger values for n should have been found had it been possible to include also particles of shorter range in the countings. In our experiments the values found for n Q are of the same order of magnitude as those found by Rutherford and Chadwick, save that the rise in the absorption curves near the lower limit of range, in our case 14 cm. of air, was still more pronounced. It would appear from these facts that, provided there is no lower limit of range for the *H*-particles expelled, the total number of particles, including such of very short range, if they could be counted, would give

* L. F. Bates and J. S. Rogers in a letter to *Nature* September 22, 1923, claim to have found particles of a range exceeding 14 cm. of air, apparently *H*-particles, to be expelled from radium C in air or in carbon dioxide ("Long-range Particles from Radium-active Gases").

† A full description of these experiments is being printed in the Sitzungsberichte der Wiener Akademie der Wissenschaften for July, 1923.

higher values for n/Q than would correspond to a reasonable value of the probability of a satellite hit.

TABLE I.

Element.	Max. range.	Element.	Max. range.
Lithium	10 cm.?	Neon†	...
Beryllium	18 cm.	Sodium	58 cm.
Boron	58 cm.	Magnesium	13 cm.
Carbon*	...	Aluminium	90 cm.
Nitrogen	40 cm.	Silicon	12 cm.
Oxygen*	...	Phosphorus	65 cm.
Fluorene	65 cm.	Sulphur‡	...

* Not tested for H-particles of shorter range than 9 cm.
† Not tested at all.
‡ Not tested for H-particles of shorter range than 32 cm.

The increase in the velocity of the expelled H-particles with that of the impinging α-particles seems to be the main reason why the authors quoted have discarded an alternative hypothesis for the mechanism of disintegration, which may be called the "explosion hypothesis." For at an explosion of the nucleus, where the α-particle only acts as a detonator, such a relationship ought not to exist. It appears, however, to be hardly justifiable to compare the α-particle to a detonator, as its energy is at least of the same order of magnitude as that released from (or absorbed by) the nucleus itself at the disintegration. A system of elastic forces under high tension, brought to instability by a violent shock will undoubtedly react in proportion to the intensity of the shock.

The Explosion Hypothesis.

It seems to be worth while to examine the explosion hypothesis as an alternative to the satellite hypothesis. According to the former view, the impact against a swift α-particle brings the structure of the nucleus to a state of instability resulting in a kind of explosion, at which one or, possibly, more fragments are expelled. The velocity of the fragment would be determined, partly by the momentum transferred from the α-particle to the nucleus *as a whole*, partly by the electrostatic repulsion between the positive charge on the fragment on the one hand and the excess positive charge of the residual nucleus *plus* that of the α-particle on the other; partly, perhaps, by the velocity of its intra-nuclear motion prior to its expulsion. To simplify matters, we assume that the only fragment expelled is an H-particle, and that its velocity relative to the residual nucleus is independent of the direction of its flight. Its velocity relative to an observer being then u_f for particles of forward direction and $-u_b$ for those emitted backwards, we should have

$$\tfrac{1}{2}(u_f+u_b)=U$$

where U is the forward velocity of the residual nucleus at the moment of expulsion. Considering only the full hits, at which a maximum of momentum is transferred, we have the following upper limits for U:

$$U' \leq \frac{4}{A+4} \times V$$

and

$$U'' \leq \frac{8}{A+4} \times V$$

where 4 is the mass of the α-particle, A that of the nucleus, V the velocity of the

α-particle, and U', U'' the velocities of the nucleus, at the state of closest impact and at the end of the collision respectively.

If the H-particles are expelled at the former stage, we should have

$$U' = \tfrac{1}{2}(u_f + u_b) \leqq \frac{4}{A+4} \times V; \quad \ldots\ldots\ldots \quad (1)$$

if they are expelled at the end of the collision, we should have

$$U'' = \tfrac{1}{2}(u_f + u_b) \leqq \frac{8}{A+4} \times V \quad \ldots\ldots\ldots \quad (2)$$

The values of u_f and of u_b have been measured by Rutherford and Chadwick for the six elements they have succeeded in disintegrating, and are given by them in terms of V.* The values for $\frac{1}{2}(u_f+u_b)$ calculated from their data are given in the second column of Table II., whereas the last column of the same table gives the values of $\frac{4}{A+4} \times V$. Considering the experimental difficulties, the agreement between the two last columns of the table may be said to be quite satisfactory for four out of the six elements studied. For boron the second column gives a value only half as high as that of the last column, whereas for fluorine the same ratio is about 2 : 3, so that in both cases the experimental value is considerably lower than the theoretical value given by equation (1). It would therefore appear that with four of the six elements investigated the expulsion of the H-particle occurs at the moment of closest impact, whereas with boron and fluorine it appears to occur at an earlier stage, i.e., with the two lightest of the disintegrated elements which have their atomic mass given by $A = 4 \cdot n + 3$.

As no simple relationship like that shown by Table II. is suggested by the satellite hypothesis, one may say that the only experimental data which allow of a comparison between the two alternative views seem to speak in favour of the explosion hypothesis.

TABLE II.

Element.	$\frac{1}{2}[u_f+u_b]$.	$\frac{4}{4+A}V$
Boron	$0{\cdot}14V$	$0{\cdot}27V$
Nitrogen	$0{\cdot}23V$	$0{\cdot}22V$
Fluorine	$0{\cdot}11V$	$0{\cdot}17V$
Sodium	$0{\cdot}15V$	$0{\cdot}15V$
Aluminium	$0{\cdot}12V$	$0{\cdot}13V$
Phosphorus	$0{\cdot}10V$	$0{\cdot}11V$

THE SATELLITE HYPOTHESIS AND COULOMB'S LAW.

A satellite of positive charge revolving round the nucleus at a certain distance from it necessarily involves a change in the sign of Coulomb's law for distances of the same order. This is also recognised by Rutherford and Chadwick when they say: "This implicitly assumes that positively charged bodies attract one another

* Phil. Mag., Vol. 44, p. 432 (1922).

at the very small distances involved. Such attractive forces must exist in order to hold the ordinary composite nucleus in equilibrium, and it seems likely that these attractive forces will extend some distance from the nucleus."*

Such a change in the sign of the electrostatic forces at very close distances has repeatedly been advocated in order to get over theoretical difficulties encountered in the dynamics of the atoms and of their nuclei. But on the whole the tendency of recent years has been against this view. Important experiments carried out in the Cavendish laboratory by Sir Ernest Rutherford and his collaborators on the collisions between swift α-particles and atomic nuclei have been interpreted as proving that the law of the inverse square holds good at least at distances exceeding $3 \cdot 10^{-12}$ cm.† In fact, ascribing a certain geometrical shape to the α-particle, the same law has been extended to hold even down to the distances occurring between the hydrogen nuclei and the α-particle at a head-on collision, i.e., at distances of the order $5 \cdot 10^{-13}$ cm., and, starting from this assumption, the dimensions of the α-particle have been calculated. The satellite hypothesis implies that the distance between the satellite and its nucleus must be considerably greater than that occurring at a head-on collision. Hence, if there is attraction instead of repulsion between positive charges at such intermediate distances, the sign of Coulomb's law must change not once, but twice, as the distance is reduced. In fact, it will have to change sign *three* times if there is again to be attraction between the constituents of the nucleus which carry positive charges ; or else one must assume the distances between these to be greater than those occurring at a nuclear collision.

With regard to the electrostatic forces existing *within* the nucleus, nothing very definite can be said at present. Indirect methods for finding the law of force from the effect of " packing " on the atomic weight‡ rather favour the view that the exponent in Coulomb's law is there slightly *higher* than 2, whereas a change in the sign of the force should be expected to begin with a *decrease* in the exponent.

There are, however, also other reasons for taking exception to the view stated in the second of the two sentences just quoted. If we assume that the composite nuclei owe their stability to attractive forces between the positive electric charges of their constituents, then there is, of course, no necessity, from the stability point of view, for any negative charges to enter into the combination. A structure consisting exclusively of two or several " protons " might then have all the stability required, whereas no nuclei of this composition are known to exist. Indeed, it is a significant fact that the ratio in numbers between the nuclear electrons and the protons is nowhere found to be less than 1 : 2, and for the elements having a nuclear charge exceeding 74, not less than 3 : 5. It would therefore seem reasonable to regard the nuclear electrons as components essential for the stability of the combine, acting as a kind of cement which keeps the excess positive charges together in a stable structure. That such structures can be imagined for the lighter nuclei is proved by Lenz's model for the helium nucleus. For the more composite nuclei models based on similar principles can also, no doubt, be

* Phil. Mag., Vol. 42, p. 824 (1921).

† Phil. Mag., Vol. 37, p. 437 (1919); Vol. 40, p. 736 (1920); Vol. 42, p. 892 and p. 923 (1921).

‡ " Uber Rutherford's X_3 und die Abweichungen vom Coulombschen Gesetz, &c.," by A. Smekal Sitzungsberichte der Ak. D. Wissenschaften in Wien, Abt II*a*, 130 Bd., 3 and 4 Heft (1921).

imagined,* although the difficulties encountered in calculating their exact configuration and the conditions of their stability may prove insurmountable.

We may, in conclusion, say that, from a theoretical point of view, a model for the structure of the atomic nuclei and for the mechanism of their disintegration which leaves the sign, if not the exponent, of Coulomb's law unaltered is to be preferred to the satellite hypothesis.

Conditions for Disintegration.

From the satellite hypothesis standpoint four conditions for disintegration have to be fulfilled :—

(1) A disintegrable nucleus must possess an external satellite revolving outside the main nucleus.

(2) The distance between the satellite and its nucleus must be sufficiently great to allow of the α-particle transferring a considerable part of its momentum directly to the satellite without affecting the nucleus to a corresponding degree.

(3) The charge on the nucleus, and the repulsive force it exerts on the α-particle, must not be too great for the latter to advance against it right up to the satellite with sufficient momentum remaining to cause its expulsion.

(4) The orientation of the impact and the time when it occurs must be such as to make the α-particle collide with the satellite when the latter happens to be at a part of its orbit favourable for expulsion.

As has already been pointed out, the first three of these conditions would make disintegrability a rather exceptional quality limited to a small fraction of the different kinds of atoms. Notably would condition (3) make the nuclei of the heavier atoms invulnerable to the swiftest α-particles, on account of their relatively high nuclear charge.

According to the explosion hypothesis, on the other hand, disintegrability should be a general property, common to the nuclei of *all* atoms except, by definition, the hydrogen nuclei, the protons. The only condition for disintegration would be :—

That the momentum transferred to the nucleus as a whole from the impinging α-particle exceeds or equals a certain critical limit D, which may be assumed to have a different value for the different elements.

Considering only those collisions at which the momentum transferred is a maximum we have as condition for disintegration $D \leq 4V \times \frac{A}{A+4}$ if the disintegration occurs at the moment of closest impact, as appears most probable according to Table II. ; or $D \leq 8V \times \frac{A}{A+4}$, if it occurs in the final stage, and the collision is assumed to be perfectly elastic, V being the velocity of the α-particles used in the experiments.

The question at once arises, whether the quantity D, defining the stability

* Also for the nuclei of atoms having $A = 4n$, like carbon, oxygen, &c., which have been assumed to be composed exclusively of helium sub-nuclei, an assumption which is certainly open to argument, the mutual distances between these are no doubt small enough to allow their nuclear electrons to exert their cementing influence on the protons of the other sub-nuclei.

of the nucleus, is always the same, or whether it varies with time for the nucleus of an individual atom. The latter supposition would appear to be the more probable. For, assuming the constituents of the nucleus to be in a state of perpetual and enormously rapid motion, it would be very difficult, at any rate with the more composite nuclei, to imagine a structure the dynamical stability of which would not undergo appreciable variations with time. Applying this reasoning to the nuclei of the radioactive atoms, as was first done by Lindemann,* we must assume the stability-function D to fall to zero, or even to take on negative values, at the moment of spontaneous disintegration, but to reach repeatedly and long before that relatively low values, corresponding to more or less semi-stable configurations of the combine. With the ordinary non-radioactive atoms the stability function never falls to zero, or at any rate only after a time so extremely long that we fail to observe it; but also with these nuclei it may be assumed to vary with time between certain limits. If, then, for a given kind of atom the stability-function of their nuclei has an *average* value greater than that defined above as the condition for their disintegration, there may still at any given moment exist a certain fraction of its atoms, $1/f$, which have a stability lower than the critical limit, and these will then be disintegrated at the head-on collisions with the impinging α-particles. If the number of such collisions occurring during a certain time is taken to be m, and the number of disintegrated atoms is, as before, taken at n, we should have $n/m=1/f$; that is, a determination of the ratio n/m with different velocities of the α-particle would serve to define the stability-function D.

The preceding reasoning illustrates how an excess in the number of the nuclear hits over the number of disintegrated atoms, if found to be real, may be accounted for also by the explosion hypothesis.

Electrostatic Forces at Nuclear Collisions.

We have so far considered disintegration to be due only to the impact of the swift α-particle against the nucleus *as a whole*, somewhat in analogy with the thermal dissociation of a composite gas brought about by the impacts of the intermolecular collisions. The main difference would be, that the dissociation of the nuclei would begin at a "temperature" of several hundreds of millions of degrees absolute, corresponding to the enormous relative velocities at the impact against the α-particles. In all probability the disintegration of the nucleus will, however, be a more complicated process, the impinging α-particle exerting selective impulses of opposite direction on the positive and on the negative charges within the nucleus. Upon the complicated field of force binding these together will be superimposed the electrostatic field due to the charge on the approaching α-particle, causing a tendency for the positively-charged constituents of the nucleus to draw away from the latter; and an opposite tendency in case of the nuclear electrons. Assuming the former to consist partly of free protons, partly of composite sub-nuclei made up from protons and nuclear electrons, say, X_3-particles or helium nuclei, the tendency to become repelled from the α-particle must be stronger with the protons, owing to their higher value of e/m. Still stronger must be the tendency for the nuclear electrons to become attracted towards the α-particle. Considering the important part

* Phil. Mag., Vol. 30, p. 560 (1915).

played by the electrons in securing the stability of the nucleus—as a kind of cement keeping the positive charges together—and considering also that they are relatively few, even a small shift of one or more of these electrons towards the point of impact may endanger the stability of the structure and increase its chances of exploding under the shock it receives *as a whole* from the α-particle. We are, therefore, justified in assuming that a selective action, due to the electrostatic forces surrounding the α-particle, causes relative displacements of the constituents of the nucleus it hits, which are to some extent instrumental in bringing about the disintegration. It is, however, clear that such a selective action, which is largely dependent on the proximity at the impact, should in general be more important with the lighter than with the heavier atoms.

Lines for Experimental Research.

The reasoning set out in the preceding pages may prove useful by the indications it affords as to which problems should first be attacked by experiment in order to decide between the alternative views on disintegration which have been considered here. It would appear desirable in the first place :—

1. To investigate which elements of the periodic system are disintegrable with our present resources, i.e., with α-particles from *Ra-C* and *Th-C*. In this investigation one should attempt to observe *H*-particles of *all* ranges and also eventual atomic fragments of greater mass, say, X_3-nuclei, *He*-nuclei, or the residual "recoil-nuclei." The methods developed by the author and Dr. Kirsch already permit such investigations to be carried out for *H*-particles of a range barely exceeding that of the impinging α-particles. Working in a vacuum and with a strong magnetic field, it will be possible, by the same methods, to observe also particles of still shorter range. Their mass can, of course, best be determined through combined magnetic and electric deflection, say, by an arrangement analogous to that of the mass-spectrograph of Aston.

2. To determine, with one or more typical elements of those found disintegrable, the total number of expelled *H*-particles, including those of very short range, so as to determine the exact value of the quotient n/Q, and also to find whether there exists a *lower* limit to the velocity of the expelled particles, or not.

3. To investigate how the quotient n/Q varies with the velocity of the impinging α-particles, with the view of determining the stability function of different atoms.

4. To determine the distribution of *H*-particles over the different directions of flight relative to that of the impinging α-particles, both as regards their number and their velocity, mainly with the view of finding the exact values for u_f and u_b, so as to test the validity of equation (1) on page 197.

5. To investigate whether one or more *H*-particles are expelled from the same disintegrating atom. This can probably best be done by means of the cloud method of C. T. R. Wilson, modified by Shimizu.

The problems concerning the structure of atomic nuclei are the most fundamental as well as the most obscure of all problems bearing on the ultimate constitution of matter. Indirect methods in which the stability of the nuclei is considered, either from the relative abundance of the different chemical elements in Nature, or by

means of calculating the effect of "packing" on the atomic weight, can hardly be expected to lead further than to more or less vague conclusions of a general character. The best line of advance undoubtedly lies along the road of direct experiment on nuclear collision and the disintegrations arising therefrom, which has been inaugurated by Sir Ernest Rutherford and his collaborators.

DISCUSSION.

Prof. C. N. da C. ANDRADE: I have listened with great interest to Dr. Pettersson's Paper, but I think it would have been of greater value if some of the experiments foreshadowed at the end had been performed before the theory was propounded. We know so little of the structure of the nucleus of even the lightest elements that the time seems hardly ripe for a decision between the two theories put forward, especially as the distinction between the satellite and explosion theories is mainly a question of words at the present stage. We know, from the energy of the released proton, that the α-particle must exercise some kind of trigger-action in the cases under discussion, but whether the released proton comes from inside or outside the nucleus is mere speculation. The only experimental evidence put forward is that given in Table 2, where the agreement between the second and third columns is not very striking. The satellite hypothesis is merely tentative in the absence of sufficient experimental knowledge, but in any case it does not necessarily imply a direct hit between an α-particle of normal size, and a proton, for deformation of the α-particle takes place in such circumstances so that the value of n/Q does not necessarily involve the validity of the theory as is suggested. The claim that the explosion hypothesis makes it possible to preserve Coulomb's law counts for little, since Coulomb's law cannot account for the stability of a nucleus composed of positive and negative charges, which when free would be about as large as the nucleus itself. The position is much as if a man having measured up a box and guessed from shaking it that it contained pieces of metal were to start speculating on the dates of the coins inside it. The appeal of the explosion hypothesis is to what may possibly be found in future experiments rather than to anything now known.

The new piece of information which has been brought forward is that, it is claimed, the nuclei of Be, Mg, and Si undergo disintegration on bombardment. If this is confirmed it is, of course, important, but it furnishes a poor basis for this load of speculation.

Dr. J. H. VINCENT: The letter of Bates and Rogers to which Dr. Pettersson gives the reference was written in consequence of one from Kirsch and Pettersson of September 15 in the same journal. They consider it possible that the particles observed by Kirsch and Pettersson may be α-particles from the active deposit. It seems that the view of Bates and Rogers on the particles of 18 cm. range which they found, and which Dr. Pettersson mentions in the footnote, was that they were probably due to "natural" H-atoms, and not to the products of disintegration.

If it should turn out that the view advocated in the Paper is true, and that all atoms are liable to disintegration, it would provide a simple explanation of a very puzzling circumstance in connection with the possibility of deriving all the elements from some parent source. If an H-particle could be removed by bombardment from a member of a radio-active series, the residual nucleus might still be radio-active, and would then take its place in another radio-active family, such that its members would have atomic weights distinguished from those of the family to which it originally belonged by the remainder, on dividing by 4, being one less than for the original series. Thus an isolated radio-active atom would give rise to a pure progeny, while a collection of radio-active atoms originally all belonging to one family would become contaminated by the other three families possible on this hypothesis.

The Author refers to the problem of "packing." The theoretical source of available energy provided by condensing hydrogen into helium arises from the view that mass and energy are equivalent, 1 gram being c^2 ergs. It seems that it might be possible to test the matter experimentally. Hydrogen under pressure and in an enclosure at a high temperature should, if even a small fraction changes into helium, take up a temperature higher than the walls of the enclosure.

The problems of disintegration may possibly be amenable to study by means of calorimetry. Thus a radio-active source might be found to yield a less supply of heat when pure than when mixed with an element capable of disintegration.

The power of yielding long-range H-particles on bombardment is one property of an atom which seems to depend to a large extent on whether its atomic or Moseley member is odd or

even. Aston gives other examples of this interesting connection. For tabulating such properties I find it very convenient to draw out the periodic table in such a way as to separate the odd and even Moseley numbers. Thus in this scheme for the first 18 elements :—

H							He
1							2
Li	B	N	F	Be	C	O	Ne
3	5	7	9	4	6	8	10
Na	Al	P	Cl	Mg	Si	S	A
11	13	15	17	12	14	16	18

those from which Rutherford has obtained long range H-particles, viz., 5, 7, 9, 11 13 and 15 are grouped together.

Capt. C. W. HUME : If the impact of α-particles has a trigger effect on light atoms, it would seem that a similar effect might be expected when α-particles impinge on heavier nuclei which are known to be unstable or explosive, viz., those of radio-active elements. In this case should not the rate of decay of such elements be increased by bombardment ? For instance, radium emanation should have a shorter decay period when compressed than when rarified, since there would be more frequent impacts in the former case. No doubt it would be difficult to detect so small an effect, but if such an experiment could be devised it would throw light on the question raised by the Author.

AUTHOR'S reply (communicated) : The purpose of the present Paper is not to prove that the satellite theory is wrong and the explosion theory right, but to show that the second view agrees quite as well as the other, if not better, with the few experimental data available at present. It does not appear to be altogether unnecessary to point this out, considering that the satellite theory has already become introduced into text-books on atomic structure without any attempts at criticism.

As to the discrepancies between the observed and the calculated values in Table II. (for boron and fluorine), they are of the sign to be expected if we make the very reasonable assumption that the amount of force required for causing instability of the nucleus should in general be least for the lightest atoms—i.e., for the nuclei which have the smallest nuclear charge.

That modifications may have to be introduced into Coulomb's law when applying it to the electrostatic forces *within* the nucleus is quite likely, but at present there appears to be little reason for assuming its sign to become reversed. And for extra-nuclear distances, such as those at which the H-satellite might be assumed to revolve, the present experimental evidence is directly opposed to such a change of sign, which is, on the other hand, the fundamental assumption of the satellite theory. Not for the first time, therefore, this change of sign appears to have been resorted to somewhat prematurely. Whether it will ultimately have to be adopted or not may be expected to come up for discussion when the final question is faced : What happens at a head-on collision between a swift β-particle and a proton ?

A second purpose of this Paper has been to direct the attention of other experimenters to some problems which appear to be of considerable importance to our views on nuclear structure. This objective may seem to have some justification, considering how surprisingly little experimental work has been done within this most central field of research during the five years which have elapsed since it was first opened by Sir Ernest Rutherford. For, if I may use the descriptive metaphor adopted by Prof. Andrade, even if we cannot hope to ascertain the date of the coins within the box, our only chance of getting to know anything at all about them seems to lie in shaking the box as thoroughly as possible, both by experiments and by speculation. Regarding the work of Bates and Rogers, to which Dr. Vincent has referred, experiments which have just been concluded prove that only a few per cent. of the number of α-particles assumed by them to be expelled from the radium C atoms are observed when the normal α-particles from RaC, instead of being allowed to pass either through mica or through different gases, as in their experiments, are stopped by thin metal foils of gold or copper placed immediately before the radioactive source. The few particles found under these conditions are probably also secondary particles arising from gases occluded in the metal. Other experiments have led to a method for measuring the ratio in brightness between scintillations due to H-particles and to α-particles from polonium. The value of this ratio for " natural " H-particles from hydrogen was found to agree closely with the same ratio for particles expelled from quartz under bombardment with α-rays, proving that the latter particles, as Dr. Kirsch and I have assumed, are really hydrogen nuclei from exploded

atoms of silicon. The results of these as well as of other experiments dealing with the expulsion of H-particles from the light elements will be published shortly.

The remark made by Captain Hume raises a point of particular interest. It is quite possible that the rate of decay of a radioactive substance is accelerated by the impact against a swift α-particle. Theoretically the disintegration constant should then to some extent be dependent on the concentration of the substance. The number of atoms prematurely exploded cannot, however, be expected to exceed the number of nuclear hits. Even if all the α-particles discharged were absorbed within the substance itself, the maximum change in the disintegration constant from infinite dilution to maximal concentration would be of the order 10^{-4}. With emanation which is available only in very minute quantities it would necessarily be much less. On the other hand, with a long-lived substance like uranium I which gives rise to a short-lived product, uranium X, of a characteristic radiation easy to distinguish from that of the mother substance, the acceleration in the rate of decay produced by intense α-bombardment from, say, emantion with its after products, might be quite observable by ordinary methods. Experiments in this direction were started some time ago, but have not yet led to any definite results.

XI.—THE EFFECT OF A MAGNETIC FIELD ON THE SURFACE TENSION OF A LIQUID OF HIGH SUSCEPTIBILITY.

By WINIFRED L. ROLTON, *B.Sc.* and R. STANLEY TROOP, *B.Sc.*, East London College.

Received November 9, 1923.

(*Communicated by* A. FERGUSON, *D.Sc.*)

ABSTRACT.

A method of measuring surface tension recently described by Dr. A. Ferguson has been applied to test solutions of ferric and manganese chlorides for the effect mentioned in the title. The results are negative.

THE effect of a magnetic field on the surface tension of liquids has received very little attention, the only experiments with which we are acquainted being those due to Jager.* He states that no change of surface tension has been observed in various pure liquids and solutions placed in a field of 18,000 Gausses. The surface tensions were measured by determining the maximum pressure required to release a bubble from the end of a capillary tube plunged vertically into the liquid, and the results are not wholly satisfactory, as no details of the sensitiveness of the apparatus are given. The investigation is of importance, since, if a magnetic field had any effect on the surface tension of a liquid, this would affect the results in the measurement of susceptibilities by the wide and narrow tube method (Quincke).

The corresponding problem of the effect of an electrostatic field on surface tension has been studied by several experimenters, with very variable results.†

The present experiment was undertaken, as we had at our disposal an apparatus for the measurement of the surface tension of very small quantities of liquid;‡ this made it possible to reduce to a minimum errors due to the effect on the liquid of a non-uniform field.

In the experiment, a small quantity of the solution contained in a capillary tube is placed between the poles of an electro-magnet. This tube is connected to a pressure apparatus, and the pressure required to make the meniscus AB plane is measured by means of an aniline manometer (Fig. 1 (*a*)).

The surface tension is then calculated from the formula :—

$$T=g\cdot\frac{r}{2}(l_1h_1+l_2h_2)+\frac{gl_1r^2}{6} \quad \ldots\ldots\ldots\ldots (1)$$

where r = radius of capillary tube,
l_1 = density of solution,
h_1 = length of column of solution,
l_2 = density of manometer liquid,
h_2 = difference in levels of manometer arms.

* G. Jager, Akad. Wiss. Wien, Sitz. b. 108, pp. 1499-1509 (1899).

† S. J. Barnett, Phys. Rev., 6, pp. 257-284 (1898). (Decrease in surface tension of water and mercury.) A. Sellerio, N. Cimento, 11, pp. 297-304, May (1916). (Increase in surface tension of vaseline and of olive oil.) Perucca, N. Cimento, 23, pp. 191-198, Feb. (1922). (Increase in surface tension of oil.) Merritt and Barnett, Phys. Rev., 10, pp. 65-73 (1900). (No effect on surface tension observed.) Fortin, Comptes Rendus, 140, pp. 576-578 (1905). (Effect, if any, less than 1/450.) Michaud, Comptes Rendus, 173, pp. 972-974 (1921). (No effect observed).

‡ Ferguson, Proc. Phys. Soc., Vol. 36, Dec. 15 (1923).

The solutions used for the experiment were strong solutions of ferric chloride and of manganese chloride, which have, for solutions, very high susceptibilities.

The field between the pole pieces of the electro-magnet was measured by means of a bismuth spiral, and the measurements were checked by means of a small coil and ballistic galvanometer. In the final arrangement of the apparatus, the field varied from 15,800 Gausses at the centre of the space between the poles to 15,950 Gausses in the neighbourhood of the edge.

Readings of the pressure required to make the meniscus plane were taken alternately with the field on and off. This minimised effects due to the heating of the magnet current; observations carried out over a long series of readings showed that the temperature between the poles remained practically constant throughout the whole series. Observations made to see whether there was any change in the length of the column during a series of readings showed that the length was constant.

From equation (1) it can be seen that as h_1 is made very small, and h_2 is the only

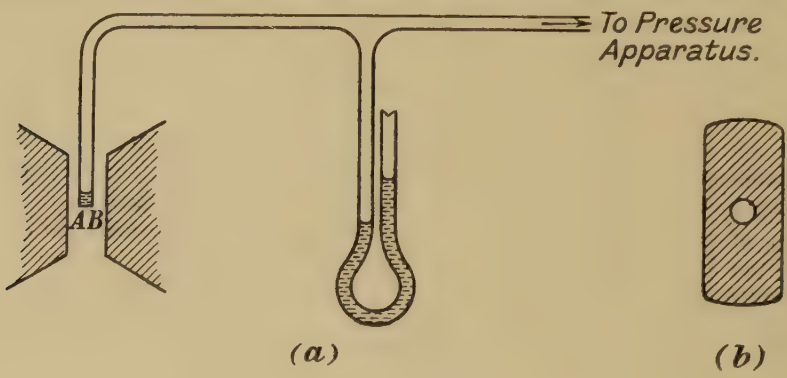


FIG. 1.

quantity which varies, any change in surface tension will be directly proportional to the change in the manometer reading, i.e.,

$$\delta T \propto \delta h_2$$

At first a small change in the manometer reading was observed; the capillary tube was then reversed between the poles of the magnet, and readings again taken. It was found that the effect was reversed, showing that it was most probably due to non-uniformity in the field.

Further to minimise the possibilities of this error, the pole pieces were pushed more closely together, so that they were less than $\frac{1}{2}$ cm. apart, and the capillary tube was ground down as shown (Fig. 1 (*b*)) until it just fitted in the space between the poles of the magnet. The thread of solution in the capillary was reduced to a minimal length (0·02 cm.), so that it could be assumed that the field strength was constant throughout the volume of the liquid.

Throughout the experiments the meniscus was viewed through a good hand lens, and was illuminated so that any change in its shape was at once evident. The field being off, the pressure was adjusted to make the meniscus plane, and the field

was then switched on, the meniscus being carefully watched meanwhile. *Not the slightest deviation from the plane condition could be observed during the numerous observations carried out.* It follows, therefore, that no change in the surface tension under a field of 16,000 Gausses is observable, and it remains to discuss the smallest change in surface tension that might have been observed.

This is most conveniently done by making a number of determinations of the surface tension of one of the solutions, and discussing the probable error of the determination. As already shown, the most important quantity under discussion is the quantity l_2h_2. Leaving out of account, therefore, any probable errors in the density determinations, we proceed to discuss variations in h_2. Thirty settings in the plane position were taken with a solution of manganese chloride, the results being as follows :—

$$l_2h_2\left(\frac{\text{gm.}}{\text{cm.}^2}\right)=3{\cdot}323,\ 3{\cdot}356,\ 3{\cdot}366,\ 3{\cdot}337,\ 3{\cdot}337,\ 3{\cdot}366,$$

3·337, 3·361, 3·344, 3·311, 3·362, 3·334,
3·334, 3·248, 3·334, 3·354, 3·323, 3·258,
3·330, 3·330, 3·334, 3·307, 3·315, 3·334,
3·346, 3·321, 3·354, 3·321, 3·252, 3·321.

Mean value is $3{\cdot}332\pm0{\cdot}00396$.

Hence the probable error (%) in the value of the surface tension is

0·12%.

The experimental conditions were such, however, that a smaller value in the change of the surface tension could have been *detected*, though not *measured*. It is difficult to estimate quantitatively a factor by which the above result may be divided, but it will probably be safe to conclude that if the surface tension of a saturated solution of manganese chloride is changed by the application of a magnetic field of 16,000 Gausses, the change is less than 0·04%.

The investigation was suggested to us by Dr. Allan Ferguson, and we wish to express our best thanks to him for his great interest and help during its progress, and also to Prof. Lees for his interest in the work.

DISCUSSION.

Dr. D Owen said that the Paper represented a careful piece of work, which seemed beyond question to show that no effect on surface tension as large as 1 in 1,000 was present when $H=20{,}000$ was used. The subject lent itself admirably to the application of Dr. Ferguson's recently published method of measuring surface tension, in which a volume of liquid no greater than some tenth of a cubic millimetre was necessary. He would like, however, to make a simple calculation which leads to the conclusion from energy considerations that no observable magnetic effect is to be expected. The effect of the field is to produce magnetic energy $(\mu-1)H^2/8\pi$ per unit volume of the specimen, over and above that produced in air. Taking $(\mu-1)$ as approximately 10^{-4} for iron salts, and $H=20{,}000$, this amounts to 1,600 erg/c.c. Assuming the thickness of the surface-film to be 10^{-8} cm., the result is to add to its potential energy per unit area the quantity $1{\cdot}6\times10^{-5}$ erg. As the surface tension is 75 erg/cm.^{2}, the change is only one part in five millions, which is evidently far beyond the power of detection.

Reference is also made in the Paper to the effect of electrification on surface tension, and this effect is also relegated to the category of the minute and unobserved. But in this case the existence of a double electrical layer of molecular thickness does permit of a sensible change in the potential energy per unit area of a surface layer, as is manifested in the electro-capillary effect.

Mr. T. SMITH illustrated the frequency distribution of the errors and the limits suggested for the probable errors, as obtained from the figures quoted in the earlier Paper by Dr. Ferguson and the present Paper. In the former case only one observation out of twenty-eight lay definitely inside these limits. He thought the conditions necessary to justify the application of the formula used were not satisfied in these cases. The uncritical use of such a formula, which gives a value of the form $\varepsilon n^{-\frac{1}{2}}$, where ε is proportional to the average departure from the mean and n is the number of observations, leads in an extreme case to the result that a value true within any assigned limits, no matter how close these may be, will be obtained if the observations are sufficiently numerous, provided they are all of the same quality, no matter how bad that quality may be. So absurd a conclusion showed that the use of the formula required justification.

Dr. P. E. SHAW (communicated): These experiments have an important bearing on the theory of the constitution of matter. The intrinsic pressure in a liquid abruptly ceases at the surface, and the unbalanced normal forces there give rise to the superficial effect called surface tension. I suggest that the authors might employ a magnetic field normal to the surface. This could be done, of course, by having vertical lines of force, the pole pieces being bored to receive the capillary tube. Probably the authors have noticed a Paper (Phil. Mag., January, 1924) on the effect on surface tension due to an electrostatic field.

Dr. A. FERGUSON (communicated): The strictures of Mr. T. Smith on the figures given in my Paper seem to be beside the mark. I confine myself to two points—implied if not overtly expressed in Mr. Smith's remarks—that the P.E. formulæ are used uncritically in circumstances where the formula is inapplicable, and that some of the observations may be "bad."

The 28 observations quoted are shown below grouped in a frequency table under two class intervals, and the frequencies there recorded afford an *a priori* probability that the normal law of error is followed. (A certain measure of common-sense is, of course, necessary in settling the limits of the class-interval).* A simple test will now show whether the scatter of the observations obeys the normal law. The mean of the figures given in the table just quoted is $1{\cdot}200 \pm 0{\cdot}003$, where 0·003 represents the probable error of the *arithmetic mean*. The P.E. of a *single observation* of the series is $\pm 0{\cdot}016$, and the ordinary theory would lead us to expect to find 14 observations between the limits 1·184 and 1·216 and 14 outside these limits. Inspection of the table shows these numbers to be 15 and 13 respectively.

1·140 1·149	1·150 1·159	1·160 1·169	1·170 1·179	1·180 1·189	1·190 1·199	1·200 1·209	1·210 1·219	1·220 1·229	1·230 1·239	1·240 1·249	1·250 1·259
1·142	...	1·163	1·171 74 78	1·181 89 89	1·191 92 92 93 93	1·202 03 03 08 09 09	1·211 14 19	1·221 23	1·233 34	1·245	1·255
1	0	1	3	3	5	6	3	2	2	1	1
1		4		8		9		4		2	

Or, looking at the matter in a still more simple way, mean and median agree to 1 part in 600, and the quartiles are at 1·189 and 1·219 respectively. If the normal law of error were followed the position of the quartiles would be given by 1·184 and 1·216. Could closer agreement be demanded?

There is, therefore, no skewness in the curve, and the scatter of the observations obeys the normal law. Hence the P.E. has its usual meaning, and its use is amply justified.

It is difficult to follow the meaning of the remarkable criterion of the quality of the observations propounded by Mr. Smith. The mean of the results, as just mentioned, is given by $1{\cdot}200 \pm 0{\cdot}003$, and Mr. Smith is exercised because one only of the observations falls within the limits 1·203 and 1·197. What quantitative deduction may be made from this result it is not easy to

* Whittaker and Robinson, Calculus of Observations, p. 165 *seq.*

say—as far as I am aware the result has no bearing on any ordinary development of statistical theory. Mr. Smith has apparently confused the probable error of the A.M. with that of a single observation.*

As regards the quality of the observations, the only test of the goodness or badness of the observations which an outside observer can apply is derived from the examination of the relation between the residuals and the P.E. of a single observation. If the observations follow the normal law, those are bad (in the sense that they should be rejected) whose residuals exceed n times the P.E. Much argument has been used concerning the appropriate value of n, and modern practice is fairly well agreed in using 5. Thus in the table those observations are bad which are lower than 1·120, or higher than 1·280. The table again emerges unscathed from this test.

AUTHORS' reply (communicated): We wish to thank Dr. D. Owen for his remarks. In reply to the latter part of his remarks, the Paper refers to the effect of an *electrostatic field* on surface tension, and not to the effect of electrification. While there may be an apparent change in the surface tension, due to electrification, when all the electrostatic forces are allowed for, the effect, if observable, will be very small. (*See* Papers by Michaud, *loc. cit.*; Gouy, Comptes Rendus, 173, pp. 1317-1319, December 19, 1921; and Wagstaff, Phil. Mag., Vol. 47, January, 1924, p. 83.)

The error in the arithmetical mean referred to by Mr. T. Smith was due to a misprint, now corrected.

We thank Dr. P. E. Shaw for his suggestion, which it may be possible to try at some future date, although the experimental difficulties in obtaining a uniform magnetic field with pole-pieces as suggested would-be very great. Possibly the method described by Wagstaff (*loc. cit.*) could be applied more easily to the problem.

* Another illustration of the futility of Mr. Smith's criterion is afforded by a Paper by Wood and Stratton (Journal Agri. Sci., III., pt. 4, Brunt, Combination of Observations, p. 42.).

DEMONSTRATION OF AN AUXILIARY OPTICAL SYSTEM FOR THE EXAMINATION OF CRYSTAL SECTIONS BY CONOSCOPIC METHODS.

By C. W. Hawksley.

ATTEMPTS have recently been made to further the original investigations of Groth and others with regard to the dependence upon numerical aperture of the number of interference rings observed when uniaxial and biaxial crystals are examined microscopically in convergent polarized light.

The apparatus about to be described had its origin in the need for enlargement of the apparent field with a minimum sacrifice of definition. The arrangement known as a Becké lens, which is in very general use, leaves little to be desired in the matter of sharpness, but the interference figures thus shown are extremely small—a decided drawback when qualitative work in counting the rings is concerned.

In the special eyepiece now under consideration the additional optical system above the microscope eyepiece is retained, but it consists of four lenses disposed as in the upper draw tube of an ordinary field telescope—namely, an erector and Huyghenian eyepiece.

By this means the image of the interference figure is considerably increased in size without appreciable loss of definition. Further magnification may be obtained by the removal of the eyelens of the microscope eyepiece. If the analysis is placed over the eyepiece of the telescope system (in preference to the usual position between the eyepiece and objective) sharpness of definition is still further increased, and the objective, moreover, is then free from astigmatism. The analysis in this position does not materially restrict the field of view.

An advantage of the system here described may be noted, and that is the whole arrangement can be fitted to an ordinary microscope without radical alteration.

In order to obtain sodium light for the observation of interference figures, or for any purpose requiring monochromatic illumination, the following method is put forward as having been found to be both cleanly and efficient.

A forked stem, such as is used to support vertical incandescent gas mantles, and made red hot in a Bunsen flame, is plunged into sodium bicarbonate. A thin layer of the sodium bicarbonate becomes fused over the surface of the fork, and this, when placed in the Bunsen flame, produces a brilliant sodium light, which will continue for many hours without attention. The process may be repeated as often as necessary, the same fork being used.

DISCUSSION ON LOUD-SPEAKERS FOR WIRELESS AND OTHER PURPOSES—(*continued* from Vol. 36, Part 2).

GENERAL DISCUSSION: Dr. W. H. Eccles, Sir Richard Paget, Capt. B. S. Cohen, G. H. Nash, Capt. H. J. Round, W. J. Brown, A. H. Davis, A. J. Aldridge, Capt. N. Lea, G. C. Norris, W. E. Burnand, Dr. H. M. Barlow, P. G. A. H. Voight.

COMMUNICATED REMARKS: C. M. R. Balbi, F. E. Smith, L. W. Wild, Dr. W. A. Aikin, L. Miller.

REPLIES TO THE DISCUSSION: Prof. A. O. Rankine, L. C. Pocock, H. L. Porter, Prof. E. Mallett, Capt. P. P. Eckersley.

DR. W. H. ECCLES: The loud-speaker of to-day is really an immense achievement and does in an almost perfect manner what could not have been done 20 or 30 years ago. By its aid the science and practice of communication is being revolutionized. It is now possible for a single voice to speak to millions of people; in every country in the globe it must now be realized that the orator, the politician or the preacher, can address audiences of a size that was undreamt of a few years ago. This is being done in two ways. In this country we have the loud-speaker combined with wireless broadcasting to carry the speaker's voice to hundreds of thousands of people; but in America the loud-speaker has also been largely developed in what is called the "public address" system. In that case a speaker may be in a great auditorium or hall, or possibly in the open country; he speaks in ordinary tones to a microphone, and the speech currents are passed through amplifiers to a large loud-speaker or a series of loud-speakers. By that means an audience of 700,000 people gathered on the spot has been enabled to hear the speech. This is bound in due course to produce in every country a profound change in our political and social conditions; the voice carries personality so very much better than the printed word. It seems to me that the loud-speaker will become the great rival of the printing press, which is not an unadulterated blessing, especially in these days when large organizations of capital can to a large extent dictate public opinion by means of innumerable journals. Let us by all means have something to compete with that, even though it may be equally liable to fall into bad hands. I thought it would be appropriate for me in opening the discussion on the technical side to point out the enormous task that the loud-speaker has to attack. Its construction is faced with such exigent demands, that one would say in advance that it would not be possible to make an instrument to meet the requirements. For instance, the periodicity of speech waves varies from 100 to 6,000 per second. No engineer attempts to make an alternator to operate over such a range of frequency. In music the frequency-range is greater; it may be from 40 to 10,000 periods per second, but in fact the ear is sensitive, in middle life at any rate, from 40 to 20,000 vibrations per second. A loud-speaker designed to give a natural effect must be capable of some such range as that. In addition, the loud-speaker which is to satisfy the ear must have an enormous range in the energy of the air pressures. The air pressures in ordinary speech have been measured, and they range from the one-

thousandth part of a dyne per square centimetre to 100 dynes per square centimetre. In other words, a loud-speaker to give a natural effect must be capable of producing a range of pressures about a millionfold. Again, the minuteness of the energy stream is very surprising ; in a quiet room every word can be heard at several yards' distance if the speaker pours into the ear of the listener a volume of sound equivalent to about 10^{-8} watt. With an orchestra it is estimated that the ratio of power required between *fortissimo* and *pianissimo* is 50,000 to 1. Speech is quite intelligible if the sound is 100 times that which leaves an ordinary speaker's mouth, or even if it is one-millionth of that power, i.e., a ratio of 10^8 to 1. This can be emphasized in the following manner : Suppose that a loud-speaker on a building emits sounds of an energy of 1 kw. At three miles, speech would be easily intelligible by the unassisted ear, supposing the ear to have an effective area of about 1 cm.^{2} for receiving direct-energy air waves. If an ear trumpet were used which would collect the energy falling on 1 *m.*^{2}, then speech would be intelligible at a distance of 200 to 300 miles. That shows how very sensitive the ear is and how much it expects. If we find that the human voice, the larynx and other organs, can make these variations of great range of power and frequency, we ought to be sufficiently encouraged to say that mechanism will, no doubt, in time be evolved to do the same. It has, in fact, been evolved. Or we ought to say that if the mechanism of the middle ear can work, it should surely be possible to make a machine that will perform similar feats, seeing that the problem is a physical one and not a physiological or psychological one. That task has been largely accomplished in the past few years. The Western Electric Company's laboratory in New York has probably done more than any other laboratory in the world to accomplish this task, and I think that it will be useful to make some reference to the work that has been carried out there. Their public address system takes speech energy into a microphone, and the currents in that microphone represent an energy of about 10^{-8} watt. After being amplified they finally reach the loud-speaker. The loud-speaker responds excellently between 200 and 1,600 periods per second, and the electrical energy put into it is 40 watts. This represents a ratio of magnification of 4×10^9 to 1. The result is so good that the voice coming from the loud-speakers in an open space, if the listener is the correct distance away, is indistinguishable from the voice of the person speaking. Now we have to compare what has been accomplished with what has been said in the various Papers read during the meeting. It seems that there are three points to be considered in improving loud-speakers. The first is that we require to amplify all frequencies equally, or substantially equally even over a wide range. In addition, the instrument must work in such a manner as to give the energy to the air equably. The second is that it must not introduce asymmetrical distortion. Helmholtz in his "Sensations of Tone," in discussing the human ear points out that it is a diaphragm which is heavily loaded on one side with the bones in the inner ear. He sets down two differential equations, and deduces the rather surprising fact that the asymmetry introduces harmonics and combination-tones. If one has those in a loud-speaker, i.e., if one has a diaphragm which is acted upon very unsymmetrically and responds to equal currents in opposite directions with unequal movements, then these combination tones will be obtained. That is what I call the second difficulty in the design of loud-speakers. The third difficulty is that impulsive sounds produce damped trains of oscillations of the diaphragm. If those are not rapidly damped out they provide another source of trouble. In conclusion, I should like to say that it is surprising to think that the

efficiency of a loud-speaker reckoned in the ordinary way, i.e., the ratio of the electrical power which is put in, to the mechanical power which is got out, is only about 0·1 per cent. If that is so, it seems to me that the improvement of the efficiency of the loud-speaker is the next task confronting the investigator and designer.

Sir Richard Paget: I entirely agree with Mr. Sutherland that audibility is a matter of precision of aural resonance rather than of amplitude. For example, whispered sounds can be heard, as I have experienced, over long distances and by large audiences, provided they are whispered properly so that the resonances are clear. Many of the human speech sounds, especially the consonants, are really essential transients. They depend not upon any one group of resonances, but upon how those resonances are changing. In other words, we recognize the shape of the frequency-time curve for that particular sound. Take, for example, the consonant sounds in "t," "p" and "kee." Those sounds differ only in the shape of the curve of approach of the resonances to the ultimate resonances of the vowel sound "ee." Similarly, the consonant in "la" is essentially a transient, and if we extend the time curve of the resonances of that sound we get what is practically a diphthong. The "l" sound is entirely lost to our ears and it becomes merely a succession of vowel sounds. From that it would follow that the ideal reproducer must be sensitive to frequency changes and, as Mr. Pocock points out, it must be free from any resonances which would cause this or that component to linger beyond its allotted span. On the question of "imitation" which Professor Rankine raises, it seems to me important that what is given in that imitation shall be true. The same applies to painting; it does not matter how much an artist leaves out as long as he does not put anything in which is wrong. Further, the scale of magnitudes may be fundamentally altered without the faithfulness being apparently affected. A constant component, e.g., the scratching of a gramophone needle, is permissible, provided it is not too loud. Ultimately the ear becomes accustomed and ignores that. An interesting example was given of the ability of the ear to take certain components for granted, in Dr. Eccles's experiments for producing artificial vowels in a telephone by means of electrical resonance. Dr. Eccles produced quite recognizable successions of vowel sounds by using only the upper components of my vowel chart, and the human ear filled in the lower resonance, provided that the upper resonances were not ambiguous. As to horns, the vice of these seems to me to lie, not in the fact that the horn possesses resonances of its own, but that it gives them an exaggerated importance when they come along in the ordinary course of speech. Mr. Sandeman's Fig. 1 refers, as I understand it, to syllabic articulation as a whole. If that chart had been limited to the syllabic consonants, such as "s," "z"; "th," "dh"; "f," "v"; "p," "b"; "k" and "g," his curves would have indicated that a much greater importance is to be attached to frequencies of between 2,000 and 3,000 as the lower limit, and 6,000 or 7,000 as the upper limit. For instance, in my own voice "s" has a principal resonance of over 6,000; "sh" has a principal resonance of over 3,000; "f" has a component of between 5,000 and 6,000; while "th" has a component of between 2,500 and 3,400, varying with the vowel with which it is associated. Similarly, "k" has an initial resonance of something like 3,000, and "t" has a resonance of between 3,000 and 6,000. On the other hand, some of the nasal resonants are down below 200. We have therefore to deal with a rather long and formidable range of resonances. Now a word as to amplitude. In "t," "d," "p," "b," "k" and "g" the difference between each of those is only, or almost

only, one of relative amplitudes. The resonances themselves are practically the same. The reproducer must therefore be faithful also to rapid changes of amplitude as well as to rapid changes of frequency. It is the high frequencies and the rapid transients which form the difficulties of speech and the difficulties of interpretation. The system must therefore be sensitive, not merely to those frequencies from 2,500 up to about 6,800, but also, for the sake of the vowels, from 200 up to 2,500. I feel confident that if faithfulness of that type can be obtained the amplitude may be enormously reduced without loss of intelligibility.

Captain B. S. Cohen: As an independent observer who has had occasion to test many of the different types of loud-speakers on the market, I may perhaps be permitted to make a few remarks on the present state of the loud-speaker. At a meeting of this Institution in the early 1900's Sir Oliver Lodge demonstrated his hornless loud-speaker, which consisted of a moving-coil electromagnetic system coupled to a wooden diaphragm, and I think it will be agreed that the articulation of that device was of a very high quality. A small number of loud-speakers of the present day are operated on this principle, but Lodge's thin and large wooden diaphragm is presumably considered uncommercial and its replacement by a metal diaphragm and the addition of a horn has not been particularly advantageous. The great majority of present-day loud-speakers have diaphragms of the usual pattern, either magnetic and operating direct on the magnetic poles or, in the largest types, linked to an armature. In spite of Professor Scripture's dictum that a diaphragm is unsound, as it is bound to produce a distorted wave-front, and that the correct device is a stiff piston, very few attempts have been made in this direction. Novel types of loud-speakers unfortunately have, so far, generally produced novel types of articulation, i.e., novel to the received wave-form. I should like to supplement what has already been said regarding the fundamental basis of operation. To get perfect articulation we require our loud-speaker to have uniform frequency/amplitude characteristics, and I would define the perfect loud-speaker as one having a ratio of acoustic output to acoustic input of unity at all amplitudes and at all frequencies and combinations of frequencies. The volume is of considerable importance, as a perfect reproduction of, say, an orchestral piece with a volume, however, of only 1/100th the original would be of little artistic and realistic value. We can produce wireless receiving amplifiers with combinations of radio, detector and audio stages which give very reasonably uniform frequency/amplitude characteristics, and we can do the same for wire telephony with combinations of loaded lines, filters and audio-frequency amplifiers. We can also employ in both cases transmitting apparatus that will very fairly follow the frequency/amplitude characteristics of the applied sounds. But what can we do with the loud-speaking receiver? We can tune out the more prominent harmonics by mechanical modifications of the moving system, by acoustic modification in the air chambers above and below the moving system and in the horn, and, lastly, we can apply electrical tuning to the loud-speaker in the form of rejector circuits and filters. But all these devices, if pushed to the point of producing a true uniform frequency/amplitude characteristic, result in the loud-speaker belying its name; in other words, there is no reasonable audio output, and the devices above referred to can only be used to the extent of effecting slight improvements. In some cases it is reasonable to infer that the use of a receiving amplifier with a uniform output will result, with a particular type of loud-speaker, in considerably more distortion of the blasting form than when a non-uniform audio-

frequency amplifier of, say, the transformer-coupled type is utilized. Thus, if a loud-speaker has a prominent resonance point at, say, 2,000 periods, and a non-uniform amplifier with a flat peak at, say, 1,500 periods is used, this might give more articulate output than a uniform-output amplifier, and will certainly give a very considerable sound output. All this indicates the importance of fitting the loud-speaker to the amplifier, and in the very few cases where this is deliberately done the results are of a very high order indeed. Many loud-speaker and amplifier combinations which give fairly uniform output from, say, 500 periods upwards, and therefore are very articulate for speech, give a colourless tonal quality to music in consequence of the rapid falling-off in output at frequencies below 500, and this is a point to which I think serious attention should be drawn. I would suggest as a very fruitful field for experiment the combination of three or four loud-speakers used simultaneously and each tuned to exaggerate slightly a different portion of the audio spectrum. It is obvious that the frequency/amplitude characteristic of loud-speakers, and indeed of any telephonic apparatus, is of fundamental importance, and apparatus for the measurement of this characteristic is becoming essential in the research laboratory. I have brought to this meeting some apparatus which has been developed for this purpose and I shall now describe and demonstrate it. The device consists of a heterodyne oscillator and high-frequency amplifier coupled to a detector, low-pass filter and audio-frequency amplifier. The combination gives a constant output over the audio range of about 50–5,000 periods, and the whole range is obtained by rotating an air condenser through 180°. This condenser is placed in a light-tight box, provided with a shutter and has a cylinder, carrying negative paper, attached to it. If the frequency/amplitude characteristic of a loud-speaker, for example, is to be obtained, the loud-speaker is fed from the oscillator and its output recorded on the negative paper by a non-resonating microphone transforming the received sound into current and recording on the negative paper by means of a suitable galvanometer. The rotation of the condenser dial by hand through 180° produces the complete frequency/amplitude characteristic of the apparatus under test. Constant output is obtained by using variable resistance and inductance combinations for coupling the audio-frequency amplifier.

[Captain Cohen then gave the following demonstrations : (1) Output of oscillator and its amplifier to give constant frequency/amplitude characteristics ; (2) output of an ordinary form of two-stage transformer-coupled audio-frequency amplifier, with very small output at low and high frequencies ; and (3) motional impedance component of a loud-speaker indicated by horn effect and hand effect at resonance points.]

Mr. G. H. Nash : I should like to put forward some practical considerations in connection with a fundamentally important part of any loud-speaker, i.e., the coupling between the vibrating mechanical system and the air. Professor Rankine remarks : " Of horns I will say no more than that they ought, if at all possible, to be dispensed with . . . because of their resonant character." Captain Eckersley, also, has demonstrated a loud-speaker having a large diaphragm and no horn, and has claimed that this receiver brings out the lower tones so often lacking in other loud-speakers. Let us consider the large diaphragm. It is inherently an inefficient arrangement ; it has great mechanical impedance directly coupled to small air impedance. It is true that receivers of this type bring out the low frequencies very well, but the diaphragm coupling is actually less efficient at low frequencies

than at high frequencies, and a good low-frequency output in practice is achieved by having a low resonance point. Such a receiver therefore possesses as much resonant properties as a horn receiver ; in fact, the low frequencies are overdone and the damping is so low that crisp, staccato effects are spoiled. The reproduction of low frequencies is not therefore an intrinsic virtue of the large diaphragm ; in fact, the same result can be secured with a horn if the diaphragm behind the horn is loaded sufficiently to have a low resonance frequency. Something might be done on the principle of the large diaphragm, if it were not inherently inefficient. Experimental work indicates that the wave-forms of speech or music have peaks somewhere between 5 and 10 times the R.M.S. value, and in music there occurs also a wide range of intensity ; this means in practice that for the best results the last valve in the amplifier should have fairly low impedance and should be used with about 6 volts on the grid and 100 volts or more on the plate when an efficient loud-speaker is used. The use of an inefficient instrument, such as the large-diaphragm type, requires not only that more valves must be used, but that the last one must

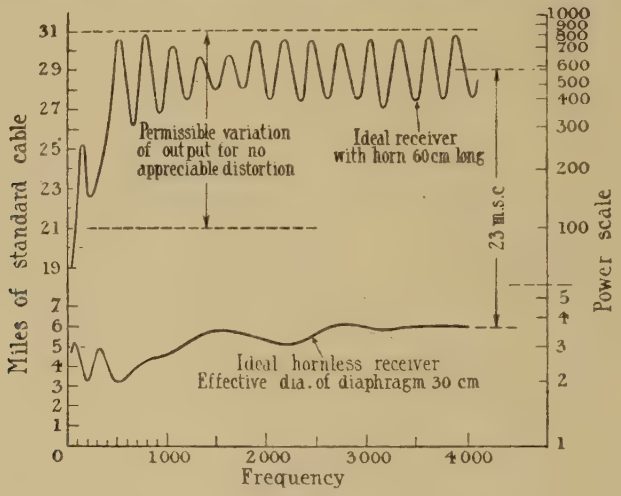


FIG. A.

be capable of a greater output of undistorted power. Now consider the horn ; it is physically a scientific and proper coupling between the impedance of the mechanism and the air ; its only fault lies in the necessity of compromising between a length that would be ideal and a length that is convenient. Let us see how serious the reputed resonance effects of the horn really are. Fig. A shows the calculated sound-power output from a large weightless diaphragm for varying frequency when a constant force is acting. The upper curve shows the output from a horn of reasonable length, having the same diameter at the large end as the diaphragm and with the same force supposed to be acting at the small end. It will be noticed that the given force produces about 150 times as much power through the horn as it does when applied direct to the diaphragm, and the resonance effects of the horn are really not at all important. It has been well established that distortion within the limits indicated by the dotted lines is quite negligible, and the resonances of the horn are seen to be well within these limits. Further, the resonances can be

reduced in amplitude and the output at low frequencies increased by using a longer horn. I contend, therefore, that horns are by no means to be discarded; on the contrary, they *must* be used if good results are to be obtained with a reasonable number of valves of reasonable power, and if a horn is properly designed it does not introduce serious distortion and is not responsible for distortion effects for which it is often blamed.

Captain H. J. Round: If we take any microphone and attempt to magnify the resulting currents, we are very quickly led to recognise the limitations of the amplifying apparatus. A valve amplifier can be stated to have certain peak-voltage limits. Distortion in a valve is generally produced by two effects: the curvature of the plate current characteristic, and the voltage-drop due to the grid current; and in order to exclude these the peak voltages of the microphone current which have to be magnified must not exceed certain values. By increasing the size and power of the valves we can increase this voltage limitation, but there is one place in the system in which this cannot be done economically, and that is the place where the maximum power is used, namely, in the wireless transmitter itself. The consideration of the effect of this is exceedingly complex. The natural sounds which we desire to transmit have all sorts of peak amplitudes, at all sorts of frequencies, but I shall now only consider the question of what basis to adopt for transmitting these different frequencies. The difficulty is this: Suppose we have a microphone which can deliver voltages at the end of our amplifier for all frequencies proportional to air amplitude, or proportional to air pressure, or proportional to some arbitrary scale. What would be the result? It will be admitted that, provided the law were known and moderately simple, we should merely have to apply at the receiving end an inverse to reproduce the original sounds, if no other considerations such as wireless interferences, &c., entered. Unfortunately, however, they do enter: the adjustment of the microphone may be such that the low notes occurring in nature produce 10 volts, whereas the high notes produce 0·1 volt, so that our valve transmitter will be fully used for the low notes and used hardly at all for the high notes. This is very important at the receiving end, for really we are getting 1 kw., say, for middle C and 0·001 kw. for the piccolo. Admittedly, given no atmospherics and no valve noises, this difficulty can be overcome, but in general these interferences will be of the same amplitude at all frequencies, and we should have the same ability to hear all the frequencies over the interferences. What exactly does this mean? On the microphones and amplifiers in use now, it is possible to make a very large number of changes in the way the frequencies are represented. Experimentally, I have tried a large variety of sounds such as orchestras, singers, speakers, &c., and I have set the microphone adjustments so that on a variety of ordinary receiving apparatus, such as telephones and loud-speakers, the results are loudest and on an average satisfying and I have then measured what my microphone is doing. Very approximately equal amplitude of modulation is being given to the transmitter from 200 frequency to 5,000 frequency for sounds of unity audibility, taking as a standard the average of a number of observers. What one might call the tilt of the scale has to be altered a little for various situations, such as different rooms; but in face of other serious difficulties, which I shall mention, this is a small matter. The sound experts will certainly object to unity audibility, but actually it is at the moment the easiest thing to get without complex apparatus. They will cite the recent work of Fletcher, published

in the " Journal of the Franklin Institute," on the masking effect of low tones on high tones, the meaning of which is really that no complex tones ever sound the same unless the strength remains the same to one's ears. This masking effect is extremely marked and at present prevents any attempt to get a perfect reproduction with a different strength from that of the original. I shall now refer to the use to which we put this. I have a very simple piece of valve apparatus which will deliver pure notes at any frequency and amplitude. I insert these currents into my telephones or loud-speaker and I plot a curve between voltage applied and frequency for just audibility. Curve 1 in Fig. B represents one of these curves for a pair of quite ordinary telephones, and the resulting effect on audibility (neglecting masking effects) is illustrated in Curve 2. The meaning of this is as follows : 2LO, received by a receiver in which there is no appreciable electrical distortion, on those telephones would give audibilities for the different frequencies as represented in the curve, instead of equal audibilities. If we try an experiment with that particular pair of telephones and insert in series with them a sufficiently damped rejector circuit so as to flatten out that curve very considerably, although we shall have reduced the total noise we shall quickly understand what fine quality we have missed without

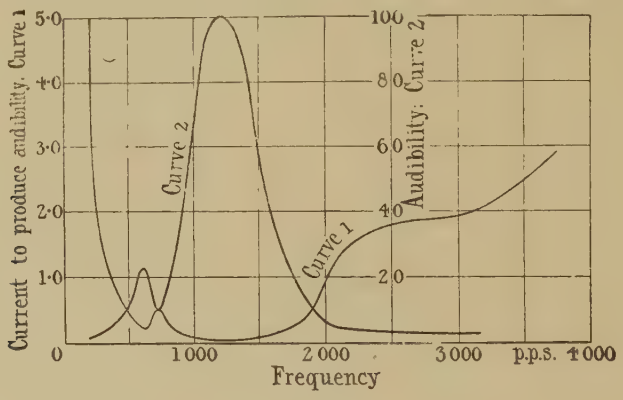


FIG. B.

it, and we can increase the total sound much more than before without hurting our ears. Moreover, that annoying room echo in some transmissions is considerably reduced. Probably before long it will be possible for the National Physical Laboratory or the British Broadcasting Company to give us a frequency strength standard and then the difficulties of obtaining the audibility curves of our receivers will be less. I have taken up this question of what is being transmitted, and a simple case of its action on a telephone, to show those who are designing telephones and loud-speakers what the input is with which they have to work.

Mr. W. J. BROWN : At the last meeting Prof. Fortescue drew attention to the necessity for using in the last stage of an audio-frequency amplifier a valve of considerable output, operating at an anode voltage of the order of 200. From the point of view of the broadcast listener the extra expenditure involved by the use of such high anode voltages is a serious item, while the use of a valve having a large electron emission causes an undesirable drain on the filament batteries. Hence it is of great interest to know exactly how much power a given valve operating at a given anode voltage is capable of delivering to an output circuit such as a loud-

speaker without introducing distortion in the valve ; this may be termed the " maximum distortionless output." In this connection the orthodox power rating of a valve is extremely misleading ; for instance, a so-called 20-watt valve will be found to have a maximum distortionless output of only 0·01 to 0·1 watt, using an anode battery of 120 volts. In course of time, valve manufacturers will probably state the exact amount of power which their valves are capable of handling without distortion when operating at some standard anode voltage, but at present the user of a valve has to make his own calculations, and I propose to develop a very simple formula whereby the maximum distortionless output can be calculated. The method has probably been used before, but apparently has not been published. The following assumptions are made : (1) That the output circuit is purely resistive—this, of course, is by no means true, but the case of reactive loads will be considered later. (2) That the wave-form is sinusoidal. Though this, strictly, is only applicable to the case of a sustained pure note, the formula will apply equally well for comparative purposes to any wave-form. Suppose that the anode battery voltage is E_0, and that the thick-line curve in Fig. C represents the (anode current)/(grid voltage)

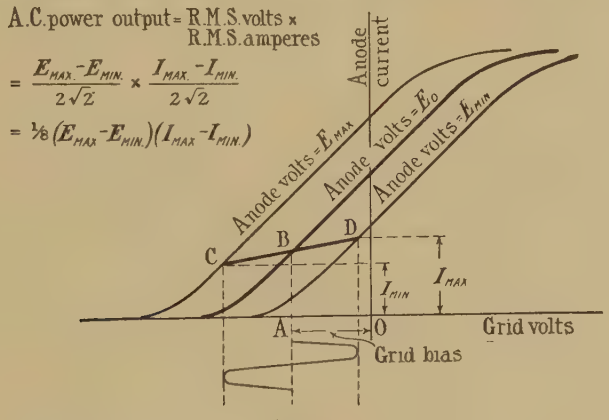


Fig. C.

characteristic of the valve at this voltage. Choose now a value of grid bias, say OA. This gives the operating point B on the characteristic. If the alternating-current voltage applied to the grid has the magnitude indicated in Fig. C, the operating point will move along some line such as CBD. The anode current will vary from $I_{min.}$ to $I_{max.}$, while the anode voltage will vary from $E_{max.}$ to $E_{min.}$, due to the resistance of the valve. The current will attain its maximum value at point D at the same moment as the voltage reaches its minimum value. The alternating-current power delivered to the resistive output circuit is obviously equal to the product of the R.M.S. anode current and the R.M.S. anode voltage, the alternating components only being considered. When the valve is working along the line CBD this power is equal to

$$\frac{E_{max.}-E_{min.}}{2\sqrt{2}} \times \frac{I_{max.}-I_{min.}}{2\sqrt{2}}$$
$$=\tfrac{1}{8}(E_{max.}-E_{min.})(I_{max.}-I_{min.})$$

By increasing the length of the line CBD we can increase the power output. In practice, the length of this line is limited, at the left-hand end on account of curvature of the characteristics, and at the right-hand end on account of grid current. In

Fig. D the line KL is drawn to indicate the highest grid voltage we may attain without serious grid-current distortion, while the line MN indicates the minimum anode current attainable without curvature distortion. In Fig. D, KL has been drawn through zero grid voltage, while MN has been drawn through the bottom of the substantially straight portion of the characteristic. For maximum distortionless output, the extremities of the line CBD will lie on the lines MN and KL respectively. The problem is to find the conditions for maximum power, and then to calculate the power output under these conditions. As shown before, the power output is proportional to $(I_{max.}-I_{min.})(E_{max.}-E_{min.})$, or otherwise to $(I_{max.}-I_{min.})(E_0-E_{min.})$. But $(E_0-E_{min.})$ is proportional to GD in Fig. D, while $I_{max.}-I_{min.}$ is equal to DN. Hence the output is proportional to the product of GD and DN. Since the sum of GD and DN is constant, the product, by a well-known theorem, has a maximum value when GD=DN. If a is the differential anode conductance of the valve, then GD on the current scale $=a(E_0-E_{min.})=\frac{1}{2}a(E_{max.}-E_{min.})$, while DN $=(I_{max.}-I_{min.})$ to the same scale. The condition for maximum output is therefore

$$I_{max.}-I_{min.}=\tfrac{1}{2}a(E_{max.}-E_{min.})$$

But $(E_{max.}-E_{min.})/(I_{max.}-I_{min.})$ equals the resistance R of the output circuit. Hence

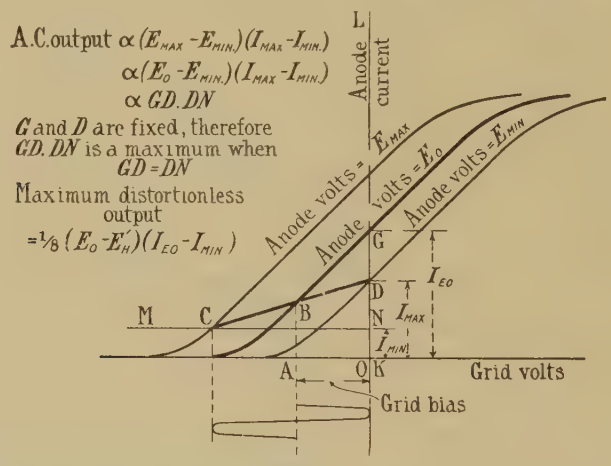


FIG. D.

$R=2/a$ is the required condition. In other words, the resistance of the output circuit must be so adjusted that it is twice the anode resistance of the valve. We will assume that the output resistance is adjusted to this value, and proceed to calculate the maximum distortionless output under these conditions. Returning to Fig. D, let I_{E0} be the anode current at normal anode voltage E_0 and at the limiting positive value of grid voltage (in this case, zero grid volts). Let $I_{min.}$ be the minimum anode current permissible from curvature considerations. Let E_H be that anode voltage which is required to give an anode current of $\frac{1}{2}(I_{E0}+I_{min.})$ working at the limiting grid voltage. This is, in fact, the particular value of $E_{min.}$ which brings the point D midway between points G and N. The maximum distortionless output obtainable is then

$$\tfrac{1}{8}(E_{max.}-E_{min.})\ (I_{max.}-I_{min.})=\tfrac{1}{4}(E_0-E_{min.})\ (I_{max.}-I_{min.})$$

But $(I_{max.}-I_{min.})=\mathrm{ND}=\frac{1}{2}\mathrm{NG}=\frac{1}{2}I_{E0}-I_{min.}$, and $E_{min}=E_H$. Hence *maximum*

distortionless output $= \frac{1}{8}(E_0 - E_H)(I_{E0} - I_{min.})$. Having chosen E_0 the anode battery voltage, the remaining three quantities I_{E0}, $I_{min.}$ and E_H, may be read directly from the (anode volt)/(anode current) curve, taken at the limiting value of grid voltage, as shown in Fig. E. We read off I_{E0} directly from the curve, at the point corresponding to E_0, while $I_{min.}$ is selected from curvature considerations. E_H is the length

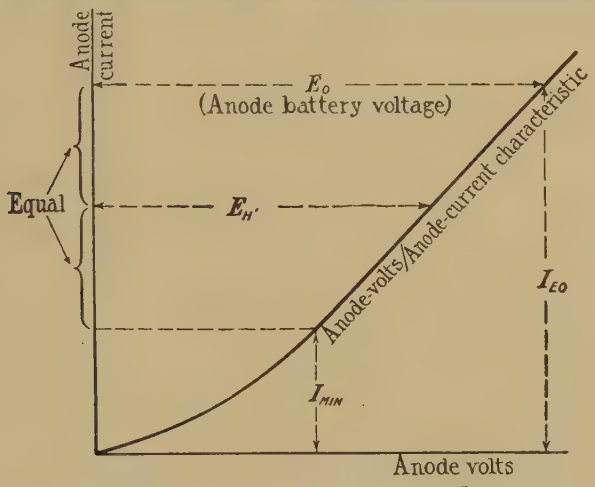


FIG. E.—DEDUCTION OF I_{E0}, $I_{min.}$ AND $E_{H'}$ FROM CHARACTERISTIC.

of the abscissa corresponding to the current $\frac{1}{2}(I_{E0} + I_{min.})$. No other data are required, so that the method has the advantage of extreme simplicity. The problem of an inductive load is more complicated and there is not time to work it out here The line CBD of Figs. C and D becomes an ellipse, and we find that the maximum distortionless output is approximately equal to the expression obtained for the

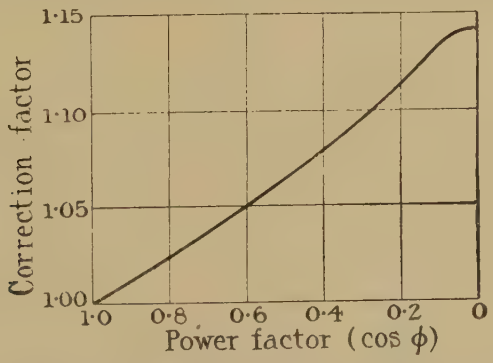


FIG. F.—CORRECTION FACTOR FOR FORMULA.
Max. distortionless output $= \frac{1}{8}(E_0 - E_{H'})(I_{E0} - I_{min.})$ cos φ.

resistive case, multiplied by the power factor. The formula thus becomes $\frac{1}{8}(E_0 - E_H)(I_{E0} - I_{min.})$ cos φ. In other words, the maximum distortionless output expresssed in volt-amperes is more or less independent of the power factor. Strictly speaking, the figure obtained is rather more favourable for low power factors, and at power factors in the neighbourhood of zero about 14 per cent. may be added to it. Fig. F

shows the correction factor plotted against power factor. It is interesting to note that the output impedance must be equal to twice the anode resistance in order to obtain maximum distortionless output. On the other hand, the condition for maximum power amplification is met by making the output impedance equal to the anode resistance. Thus we cannot have maximum distortionless output and maximum amplification at the same time. Fortunately, the impedance adjustment

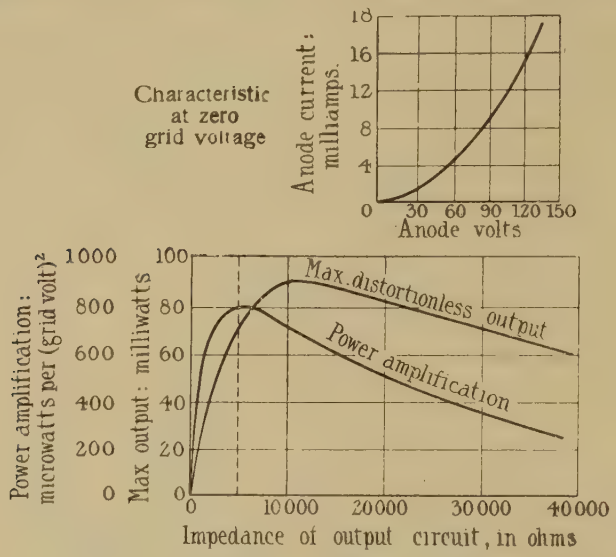


FIG. G.—OUTPUT AND AMPLIFICATION CURVES OF TYPICAL POWER VALVE.
Anode battery voltage 120.

is not very critical. Fig. G shows the relative importance of the impedance adjustment for the two cases; this refers to a valve of 5,000 ohms anode resistance. Table A

TABLE A.—*Maximum Distortionless Output in Milliwatts, and Power Amplification Factor in Microwatts per (Crest Grid Volt)².*

(1) With output circuit impedance adjusted for maximum amplification, ($Z=1/a$).
(2) With output circuit impedance adjusted for maximum distortionless output, ($Z=2/a$).

Valve.	$Z=1/a$			$Z=2/a$		
	Z	Amplification.	Output.	Z	Amplification.	Output.
	ohms.	$\mu W/V^2$	mW	ohms.	$\mu W/V^2$	mW
A	4,720	802	58	9,440	713	86
B	8,350	488	39	16,700	434	44
C	10,300	392	27	20,600	348·5	30

shows the output and amplification obtainable from three typical loud-speaker valves for the two optimum valves of impedance, using an anode battery of 120 volts. The very low value of the output figures will be noted: they vary from 0·027 to 0·086 watt, though the orthodox rating of these valves is of the order of 10 watts. In practice, the adjustment of the impedance of the loud-speaker to suit that of the valve is carried out by using an output transformer of suitable ratio or by suitably adjusting the windings of the loud-speaker. Only a very rough

approximation can be made, due to the very wide variation in impedance of the loud-speaker as the frequency is altered. It is thus fortunate that the adjustment of this impedance to suit the valve is not very critical. Figs. H. J and K show impedance/frequency curves of typical loud speakers and illustrate the amount

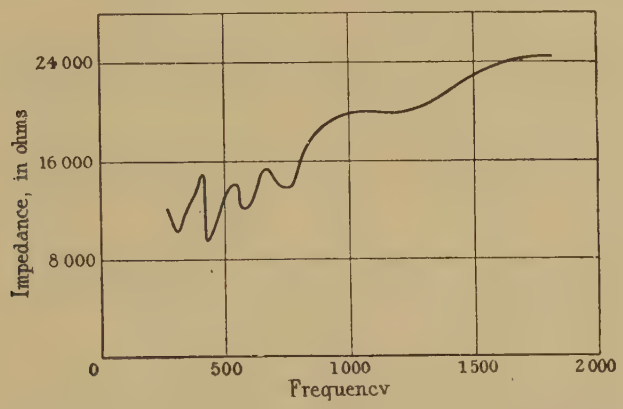


FIG. H.—IMPEDANCE/FREQUENCY CURVE OF LOUD-SPEAKER A.
Testing current 2·0 mA ; resistance 49·25 ohms at 20 °C.

of variation which may be expected. It will be noticed that these curves have very pronounced peaks, and each peak corresponds closely to a frequency at which the loud speaker resonates. In fact, this forms a very simple method of detecting the resonant frequencies of a loud-speaker. Returning, in conclusion, to the calcu-

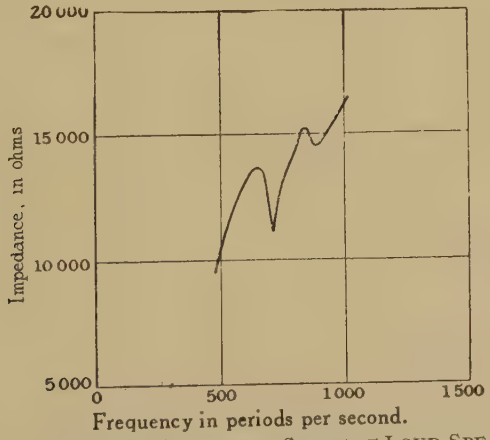


FIG. J.—IMPEDANCE/FREQUENCY CURVE OF LOUD-SPEAKER B.
Constant current = 2·5 mA.

lation of the maximum distortionless output of a valve, the formula deduced—and indeed any formula—would depend on the values chosen for the limiting maximum grid voltage and for the limiting minimum anode current. These values can best be found by aural estimation, using a cathode-ray oscillograph for determining the

limiting values reached. Prof. Fortescue has shown some cathode-ray oscillograms representing amplifier distortion. It would be interesting to hear whether he has investigated the amount of distortion, particularly of the grid current and of the curvature varieties, which may be introduced into an amplifier without detection by the human ear.

Mr. A. H. Davis : Given physical measurements of the output of loud-speakers, it remains for us to interpret the meaning of the results in terms of the effect on the human ear. Unfortunately, sufficient data for this are not available. It is, however,

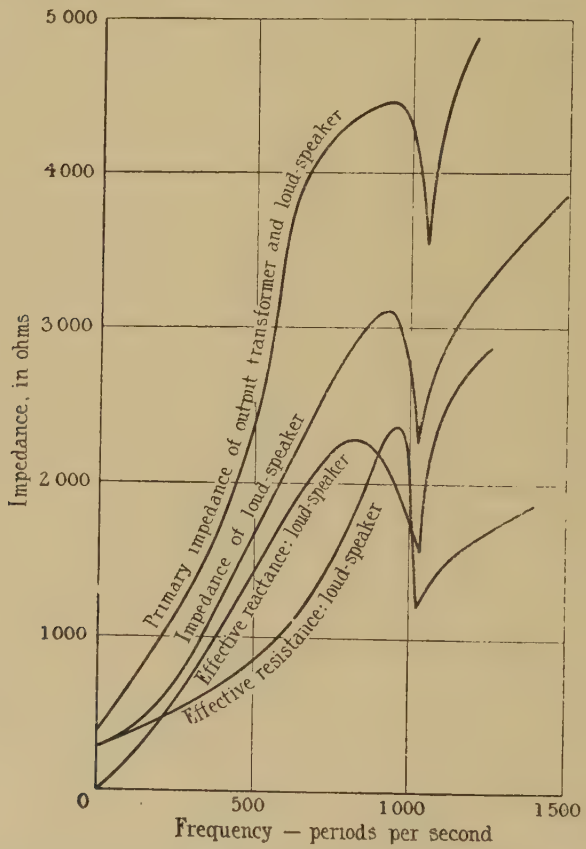


Fig. K.—Impedance/Frequency Curves of Loud-Speaker C.
Constant current = 5 mA.

desirable to consider some of the acoustic determinations which enable us to appreciate the range of the ear and the way it behaves, and especially to consider those which throw light upon any side of the question of distortion. In normal conversational speech the pressure amplitude in the sound waves at 1 ft. from the mouth appears to be about 1,000 times as great as the minimum necessary for audition, and about 1/1,000th of the maximum that can be reached before tickling sensations and pain supervene in the ear. The total range of audible amplitude

is thus about a million-fold. It assists in appreciating these figures to note that persons not ordinarily called deaf may require up to 100 times the normal "threshold" value, and those who need a further hundred-fold increase are sufficiently deaf to require an ear trumpet. With respect to the capacity of the ear for detecting differences of loudness, it just discriminates between two successive notes of the same pitch when the amplitudes of vibration differ by 5 per cent. Thus equal increments of sensation result from equal increments in the logarithm of the stimulus. It is thus useful to plot intensities logarithmically. The figure of 5 per cent. is, however, increased for faint sounds. It is found from these data that the million-fold auditory range can be covered in not more than 270 steps of increasing loudness, each one just perceptibly louder than the former. A two-fold amplitude change is equivalent to about 14 such steps, a hundred-fold to 94. The loudness of speech has some bearing on its distinctness, which is best for the medium level (1 dyne/cm.^{2}) of normal conversation. Ten-fold increase or decrease in amplitude is almost without effect, but decrease to 1/100th reduces articulation by about 50 per cent. We find that consonants are more seriously affected than vowels by reduction of speech intensity. As stated by Sir Richard Paget, some of them are carried by very high frequencies, and in ordinary conversation 50 per cent. of mistakes of interpretation can be traced to three of them alone. In these circumstances, and seeing that experience leads public speakers actually to emphasise their consonants, it is surely unfortunate that loud-speakers become weak at high frequencies. It must surely limit their use in acoustically difficult conditions unless the electrical system is designed to effect some compensation. With further reference to speech, the falling off of loud-speaker efficiency at frequencies less than 300 should have little effect on distinctness, but some change of character or raising of the pitch is to be expected. The normal male voice has a very pronounced low-pitched constituent of frequency about 120, and the female voice has one about an octave higher. To reproduce speech and to preserve all its characteristic qualities, frequencies from 100 to above 5,000 must be delivered with approximately the same efficiency. The amount of distortion permissible in reproduction of speech or of music will naturally depend upon circumstances, and no accepted criteria seem to be available. However, judging from threshold values—and there is some evidence to indicate that we may do so—there are certain imperfections in individual ears which probably set a limit beyond which accuracy of reproduction need not be carried. It is found that, although the average of several normal ears has fairly smooth sensitivity over the frequency range, the threshold sensitivity of a normal individual ear is not uniform, but exhibits maxima and minima at various frequencies peculiar to that particular ear. In a typical case, from two-fold to six-fold variations occur as the frequency range is covered. Different individuals have quite different characteristics, so that, apparently, music does not sound the same to different persons. Consequently, while the effect of distortion within, say, a two-fold limit might be detected, probably there would be divergencies of opinion as to whether improvement in quality had been effected. The *minor* peaks and resonances exhibited by the best loud-speaker curves appear to fall very near to the minimum thus indicated as the least distortion that normal individual ears could fully appreciate.

Mr. A. J. Aldridge: I wish to give some quantitative information in regard to the orders of magnitude occurring in loud-speakers, and to draw attention to

a fundamental error which appears in many commercial instruments. Fig. L shows the diaphragm motions of a number of commercial loud-speakers operated with different inputs. The first point to be noticed is the very small extent of the motion. An average of about 0·05 watt in most loud-speakers will give sufficient volume comfortably to fill a normal private dwelling room, though this does not mean that this figure may not be considerably increased on individual notes. The corresponding diaphragm motion is usually less than $\frac{1}{2}$ mil. Curves (*a*), (*d*) and (*e*) are for instruments constructed on the lines of an ordinary receiver, i.e., with a plain iron diaphragm. It will be noticed that the inwards motion considerably exceeds the outward for the same input (in the case of (*e*) by 30 per cent., with an input of 0·05 watt). This means that very serious distortion, producing harmonics, will occur, and would appear definitely to bar this type of instrument for accurate reproduction for any but small inputs. Curve (*b*) is for a moving-coil instrument, and curve (*c*) for one with a balanced armature. The apparent greater efficiency of

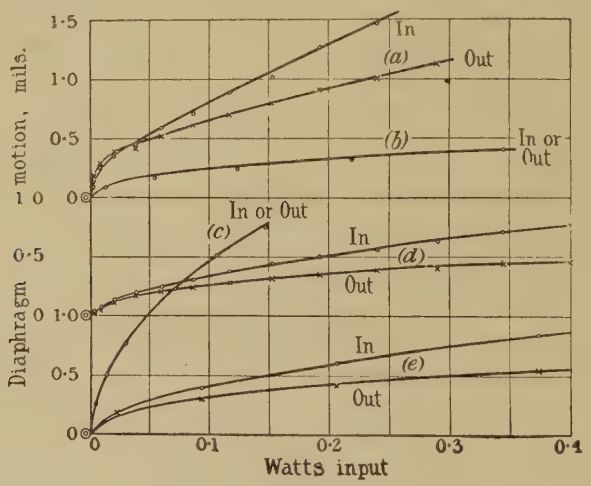


FIG. L.—DIAPHRAGM MOVEMENTS IN LOUD-SPEAKERS.

this latter model is probably due to the method of test. Direct current was used, and this makes no allowance for unavoidable losses due to slight slackness in the linkages used in this type of instrument. I have intentionally omitted any mention of resonance, which is dealt with by other speakers, but in selecting a loud-speaker it must, of course, be considered. Resonance is undoubetdly serious, but can be corrected, whereas the fault to which I have referred would appear to be only capable of correction by the use of a larger diaphragm and smaller motion, or the use of a larger air-gap, with corresponding increase of power. I think that insufficient attention has been paid to irregular diaphragm motion, which probably accounts for much of the faulty reproduction and poor tone appearing over the whole gamut of frequency unless very small amplitudes are dealt with.

CAPTAIN N. LEA: Prof. Rankine draws attention to the danger of reaction complications when a forced vibration begins to influence the form of the forcing vibration. The possibility of this happening in the mechanical parts of the system has probably been less obvious to those engaged in wireless research than that it

should occur, for example, in the last stage of a thermionic amplifier. Prof. Rankine points out in his concluding remarks that in testing results it is not much use to attempt to obtain simultaneous visible records of the sound source and sound image (if one may use such an expression), because of the difficulties of attaining exact results and interpreting them in their audible equivalents. Nevertheless, in view of the fact that, as we approach more and more to fidelity of reproduction, audition becomes an increasingly difficult test to apply, I think that the process of feeding sensibly pure sine oscillations into the apparatus under test, and of watching the results by instrumental means as the frequency of the oscillations is varied, must always be followed. Prof. Fortescue has described some of the difficulties of amplifier design, but he has not exaggerated them because he has taken no account of the limitations imposed upon any design intended to cater for the domestic user. It is, for example, impossible to expect the music-lover to fill his house with high-voltage batteries, and it is taking some time to persuade people to use an adequate number of valves which are large enough to ensure good results. In discussing the horn, which forms part of the conventional loud-speaker, no one seems to have referred to the polar distribution of radiated sound energy at various frequencies. I do not know whether other observers have noticed, as I have done on frequent occasions, that even if the frequency balance is good when the ear is in a line with the horn opening, it may happen that a large percentage of the upper part of the sound spectrum is missing when the ear is moved to one side of this line. It may be of interest to describe an arrangement which I have employed with rather pleasing results. The device may perhaps best be described as a spectroscope for audible frequencies. It consists of two main portions, first a rotating spindle for the purpose of controlling the frequency of the oscillations imposed upon the apparatus under test, in such a manner that it sweeps over the whole audible scale during the revolution of the spindle. The spindle has mounted on it a variable condenser of the semi-circular vane type which is connected to one of two high-frequency oscillators employed for the production of beats, which latter, when rectified, are used as a variable-frequency source for testing apparatus within the audible scale. The second part of the device consists of a cathode-ray oscillograph with its auxiliary apparatus. By this means it is possible to cover a very large frequency range without altering the constants of the oscillating circuits by more than a very small percentage, and hence one may be reasonably certain that the amplitude of the beats produced does not vary with their frequency. There is, of course, the risk that this method of setting up a variable-frequency source may give trouble owing to interaction between the two oscillators and also to the departure from sine wave-form of the beats when rectified. It is also necessary to employ a fairly high decrement in the high-frequency circuit attached to the rectifier, in order to prevent a change of amplitude in the induced current when the frequency of one of the oscillators is changed. The application of the beat frequency of the apparatus under test gives rise to an E.M.F. on the output side which can be applied to one pair of deflecting plates in the cathode-ray oscillograph. A rotary potentiometer is mounted on the same spindle as the variable condenser already referred to, and this furnishes an E.M.F. which can be applied to the other pair of deflecting plates for the purpose of spreading out the output indications at various frequencies and thus giving a spectrum band. The potentiometer merely separates the various frequencies in the same way as a prism does in an optical spectroscope. A commutator is also mounted on the spindle in order to

prevent a second image being obtained when the condenser is passing through the second half of its revolution, and when, in consequence, the frequency band is traversed in the inverse direction. The spectrum produced consists of an illuminated band, the width of which at any point is a measure of the response experienced at the corresponding frequency. By mere inspection it is possible not only to discover faults in the distribution of response, but also instantly to observe the effect of any steps taken to improve the results. Some speakers have raised the question of the relation between the sensitiveness of the human ear and the frequency impressed upon it. Surely it would be wrong to introduce such a function into the design of a loud-speaker, because it is only by making the output a copy of the original that we can hope to deceive the listener into believing that he is, in fact, listening to the original. Any device for the correction of ear-frequency balance would (like spectacles) have to be a personal matter, and unless the arrangement could be attached to the head it would seem necessary to put the sub-normal listener with his special loud-speaker in a room by himself.

Mr. G. C. Marris : I cannot agree with all that seems to be inferred from the facts given in this discussion. As each speaker has presented his own particular problem, the general effect is that nothing can be done. Perhaps this is true so long as we make no clear decision as to whether we will have more quality or more volume. In other words, ought we not to develop two distinct types of loud-speaker, or perhaps three ? These would be, first, a general-purpose instrument, as most of the instruments on the market seem to be. They give plenty of sound for a large private room or for the entertainment of a small private party. They reproduce speech very clearly and give music which is pleasant to listen to for a short time. Only a comparatively small valve is necessary for their operation. The second class would be the new one, and would be a type of instrument working off the largest output valve practicable in a private house, suitable only for use in a small room or by persons sitting close to the instrument, with resonance of every kind suppressed to the lowest limit at the expense of volume and designed to cover a wider range of frequencies. The third class exists already in the instrument used for large halls and audiences. Since a fairly wide range in the matter of power supply is permissible with this type, the problems of design should be simplier, or perhaps it is more correct to say, less obvious. With these possibilities, more particularly Class 2, in mind, I should like to comment on the remarks of some of the previous speakers. Several have suggested that the horn is the ideal form of coupling between the electrical system and the room. It is indeed to be hoped that this is not the case. I have listened a good deal to loud-speakers, and in all cases have become very tired of the horn resonance. That it is horn and not diaphragm resonance is easily proved by removing the horn. If we are to make an electrical analogy it must be to a resonance transformer or a very short transmission line, neither of which is a good coupling device. Further, there is the fact that the actual musical instruments with wider frequency-ranges, such as the violin and piano, have no horn. The other point is in connection with valves. Having had to urge the use of valves of large output, I am very pleased to read Prof. Fortescue's summary of the various forms of amplifier distortion. I should like to corroborate his remarks in regard to the distortion due to the valve itself. Measuring the characteristics of a valve with a power output of about the maximum practicable for a private set, I found that for about 1 volt (peak) on the grid the second harmonic

was 0·4 per cent. of the fundamental, while for 5 volts (R.M.S.) on the grid the second harmonic was 1 per cent. of the fundamental. If, however, one attempts to obtain under the same conditions so much power out of, say, an R valve, the higher harmonics would be of the order of 50 per cent. of the fundamental. It seems to me, however, that Prof. Fortescue's remarks on page 272 on the output required should be qualified, as otherwise we should have to condemn, I think, unnecessarily a large proportion of existing loud-speaker outfits. Using the ordinary loud-speaker which makes use of horn resonance, an ordinary room can be pleasantly filled with speech or music if a valve having a maximum distortionless output of, say, 5 milliwatts is used. I believe that this power covers 90 per cent. of the loud-speakers in use. Here, it seems to me, is the great opportunity for the quieter, better quality type of loud-speaker referred to above as Class 2. Let us make use of the larger output valves capable of giving 50 to 100 milliwatts of power with reasonable voltages, but direct this power to the improvement of quality, not of volume. It seems quite practicable to design valves with still larger distortionless outputs at reasonable voltages, but at present there is no loud-speaker which is capable of making good use of such power in a small room. In connection with the formulæ given to-night for output, there is a very simple way of getting the figure from the constants of the valve, pointed out by my colleague, Mr. A. C. Bartlett. If v be the length of the straight part of the valve characteristic on the negative side of zero, at the particular anode voltage applied, then the R.M.S. grid voltage which can be applied to the valve without causing distortion is nearly $\frac{1}{2}v$, and the distortionless power output is therefore approximately $(m^2v^2)/16R$, where m is the amplification factor and R the anode-filament resistance, usually known as the internal resistance of the valve. It is useful to note that $\frac{2}{3}v$ is also the optimum value of the direct current negative voltage at which to work the grid.

Mr. W. E. Burnand: From what has already been said and demonstrated it is very obvious that there are many other factors connected with loud-speaker reproduction than the loud-speaker itself, and that many faults which are blamed on the loud-speaker may be traced to something else. The extra output on certain notes—similar to what is called the "wolf" note in some violins—can often be traced to the amplifier. One can imagine an amplifier with a tendency to oscillate, working near that point. When a signal comes through of the same frequency the two together will give rise to this "wolf" note. It may be the loud-speaker, but the fact remains that, in many cases, by altering connections or applied potentials it can be got rid of, thus showing that the amplifier and not the loud-speaker is at fault. There still remains the trumpet tone and the various other resonances. Apart from these, I think sufficient prominence has not been given to the effect of harmonics and overtones. All the experiments or curves connecting output with input which have been shown are with pure tones. I think that the overtones and harmonics are of equal importance, even beyond the audible range, in the effect produced by the speaker, since these form beats within the audible range. I was led to suspect this by running two loud-speakers in series, a large Brown and a large Western, about 6 ft. apart, and standing 8 ft. away from them. The input to each was exactly the same, and yet, listening for a period, it was curious how the sound appeared to emanate first from the one and then the other, and how at other times the outputs of the two were approximately the same. It does not mean that one loud-speaker takes charge of the high notes and the other takes charge of the low

notes. It seems to be a matter of the timbre quite as much as of the high or low notes, and the timbre is of course due to the various frequencies and harmonics. For instance, the sound of a flute would appear to come from one instrument, and the announcer would appear to speak through the other. The explanation that this might be due to the standing waves is I think also done away with by the fact that one appeared to take charge for a matter of some seconds, during which a considerable number of notes were sounded, and the position of those standing waves would vary with the note. I think, therefore, that one horn or one air passage cannot be the best thing for all classes of music or all tones, and that it is possible to get a much better result by having either a number of horns or a number of air passages, which might be combined in one outlet. This is shown in rough diagrammatic form in Fig. M, several horn passages of different length and configuration branching out from the sound box and meeting again at a common outlet. It was interested in the demonstration showing the modes of vibration of a diaphragm, but I cannot think that those apply even approximately to a diaphragm such as is used in the Brown instrument with a conical diaphragm, or to the Western with the concentric corrugations, or to the loud-speaker with the radial corrugation shown by Captain Eckersley. Having designed transformers for thirty years, and personally

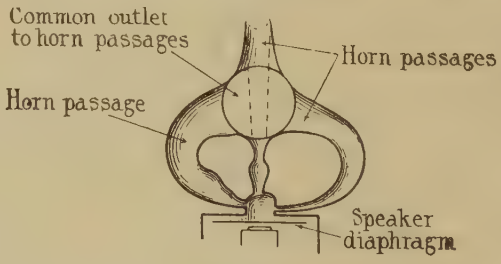


FIG. M.

evolved some 9,000 different windings, some of which are, of course, now obsolete, I must say that the average transformer put in a wireless amplifier on the low-frequency side is very much worse than might be expected from the firms who, one would imagine, should know better. On the question of the open circuit compared with the closed magnetic circuit transformer, they both have their defects. For the amplifier a transformer with characteristics lying somewhere between those of the series and the ordinary shunt transformer is required, i.e., both the magnetic and current densities vary, and also there is a direct-current component. With a closed magnetic circuit, that component shifts the flux to one side of the neutral position. Therefore the alternating component of an average of 1,000 frequency must give a distorted output. Going to the other extreme, there is the open-circuit transformer used with a straight core, which, as is well known, requires quite a large magnetising current. Many papers have been written to show that the output does not then match the input, especially under the conditions used in an amplifier, where the amplitude varies with inductive and capacity reactance as well as resistance. I think, therefore, that for good results something between the two is necessary, i.e., a transformer with a nearly closed magnetic circuit, with an air gap (a few thicknesses of paper will give sufficient spacing) of 1 or 2 per cent. of the

length of the magnetic circuit. The old Edison "electro-chemical" telephone was, I believe, the first of the loud-speakers in which the diaphragm vibration was augmented by varying the friction on a revolving element. According to an old book,* this consists of a flat spring, attached to a 4 in. mica diaphragm at one end, pressed by a rubber pad on to the surface of a revolving cylinder of moist gypsum impregnated with potash and mercuric acetate. The spring is made negative. Used with a contact point on the end of the spring touching the cylinder, this is also employed as a transmitter.

Dr. H. M. Barlow: I should like to say one or two words about a novel type of telephone which I am now engaged in developing, and which Fig. N will help to explain. The so-called Johnsen-Rahbek effect, involving the electro-adhesion set up between a solid metal and a semi-conductor which are in contact and have a potential difference between them, is well known, but I do not think it is generally known that the same effect is obtained with a semi-conductor in contact with a liquid metal such as mercury. I made this observation myself some time ago,† and I have applied it to a telephone, among other instruments. Take, for example, a piece of semi-conductor such as lithographic stone or slate, in the form of a thin disc, and make the underside of it concave or convex, so that when floated on a

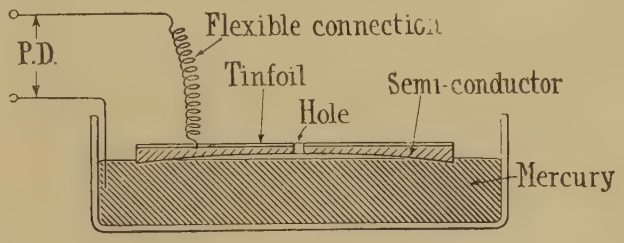


Fig. N.

pool of mercury the surfaces do not make contact over the whole area. Cement a piece of tinfoil to the upper surface of the disc, and apply a varying P.D. between it and the mercury. The semi-conductor will then oscillate on the surface of the mercury, due to the variation in the electrostatic forces operating across the interface. If a telephonic voice current is employed, speech is reproduced. An alternative method of producing sound is to bore a small hole through the centre of the disc so that, when it is pulled down, the air is squeezed out of the confined space between it and the mercury. The arrangement has given very good results with a two-valve receiving set working on 2LO.

Mr. P. G. A. H. Voigt: So far only one or two speakers have said anything in favour of the present-day loud-speaker. One of the accusations which is generally levelled against it is that of chronic inefficiency. I do not believe that the peak of efficiency is much more than 10 times the average efficiency, and I shall now show one experiment to prove that the loud-speaker's efficiency is more than the fraction of 1 per cent. with which it is generally credited. It is an experiment which has

* "Electricity in the Service of Man" (Cassell & Co.); also see H. M. Barlow: "An Investigation of the Friction between Sliding Surfaces," *Journal*, I.E.E., 1924, Vol. 62, p. 133.

† *Journal*, I.E.E., 1924, Vol. 62, pp. 143 and 156-158.

been known to telephone engineers for many years. It is a kind of Hopkinson test. The power from the loud-speaker, acting as a microphone (quite a good microphone, too), is fed through a telephone transformer to the input circuit of an ordinary single-valve note magnifier. The output goes through another telephone transformer to the other loud-speaker, and the sound output is sufficient to maintain an oscillation. If we take the extreme theoretical case of an efficiency for that stage of note magnification of 75 (transformer 5 : 1, valve amplification factor 15), then the efficiency of our loud-speakers is the reciprocal of the square root of that figure, i.e., between $\frac{1}{8}$th and $\frac{1}{9}$th of 100 per cent., say, 12 per cent. at the most efficient frequencies. If we take a more reasonable figure of 25 for the stage of note magnification, we get a peak efficiency of 20 per cent., which is very far removed from 1 per cent. It can quite easily be shown that it will oscillate on several notes, but there are definite gaps. [Demonstration.] I think those gaps explain the reason why with two different loud-speakers in series the sound apparently wanders from one to the other. There is another efficient point among the lower notes. I expect that one is the air-column resonance of the air in the horn and the first is the mechanism resonance, and I believe that the greater part of the loud-speaker distortion, when the loud-speaker is responsible for it, is due not to the horn, but to the

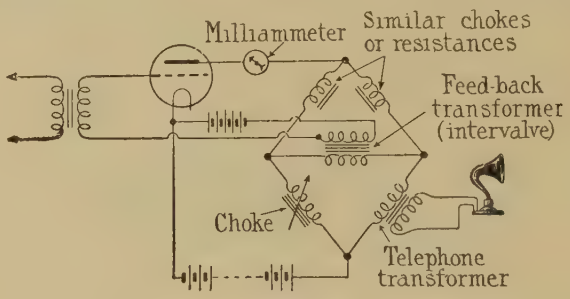


FIG. O.—COMPENSATING CIRCUIT.

mechanism. We have been told that the resonance in a telephone can be compensated for very easily by a suitable tuned circuit, and we have also been told on good authority on previous occasions that the present-day loud-speaker which is fairly well evened up cannot be so compensated. Recently I listened through my loud-speaker to the opera, and at the end of it I concluded that, although the results were very good, they were not quite natural, and I remembered a circuit which I had drawn out in May, 1922, but had not yet tried. In that arrangement a loaded telephone and a closed telephone—that is, one with the front covered up—were used in a Wheatstone bridge, and instead of having a galvanometer at the junction points I had the primary of a transformer which fed back the difference into the grid circuit, thus attempting to even up the output by balancing the loaded against the unloaded telephone. I tried that arrangement with loud-speakers, but the difference between the two is so slight that it is necessary to feed back several stages to get any change at all, and the result even then is only howling at various frequencies. But with a slight modification, i.e., disconnecting the closed loud-speaker from its transformer and reducing the impedance of that transformer, an arrangement is obtained which I think gives results a little more natural than

the usual circuits. [Demonstration.] The effect is best shown when there is a full orchestra, or preferably the opera, in which all the instruments are present. In the 2LO orchestra I understand that the bass instruments are omitted for the simple reason that they do not reproduce. Therefore a loud-speaker which would reproduce them if they were there cannot properly prove its superiority. I can switch over from the compensating arrangement to the other, but that can only give an indication of the change. If it is desired to find out what the difference really is, the characteristics of the loud-speaker must be well known. People ignorant of any change have remarked on the difference although they had not heard the instrument at work for several days. Actually the change-over on the switch does not seem to indicate anything like this difference. I should like to point out that the efficiency is only slightly reduced, and when several people are speaking together the voices appear to come out separately instead of all together. Further, the lower, and sometimes the higher, notes come out better, while the middle notes are suppressed or muffled. The compensating circuit is shown in Fig. O. The musical scale with its harmonics covers 10 or 11 octaves. The ordinary loud-speaker is fairly efficient over the two octaves which are most used (say 300 to 1,200 cycles per second). The resulting distortion is most noticeable with a piano. The human voice sounds high-pitched; an orchestra sounds as though the instruments were differently distributed; and most solo instruments sound like similar instruments of different tone. Because of this, the perfect loud-speaker, or a perfectly compensated loud-speaker, will not seem much more natural than those we have at present, unless the reproduced music can be directly compared with the original. After knowing a certain loud-speaker for a long time, however, the ear becomes very sensitive to its imperfections, and notices any improvement at once, although, to a stranger, both reproductions may sound equally perfect. The question of transformer distortion has been raised. I find that with reasonably good transformers the distortion is hardly perceptible even with a compensated loud-speaker. One great difference between a resistance-coupled and a transformer-coupled amplifier is that if we attempt to overload the valves of a resistance-coupled amplifier it shuts down, whilst the corresponding transformer-coupled amplifier distorts. Instead of the operator, it is the transformer or loud-speaker that is blamed. In conclusion, I greatly regret that many firms are now incorporating the parallel condenser in their loud-speakers. This condenser, by reducing the efficiency at the highest frequencies, not only spoils the reproduction, but by masking valve distortion encourages users to overload their valves and spoil the reproduction still further.

Mr. C. M. R. Balbi (*communicated*): Professor Fortescue's work on the power amplifier has shown that distortion is not due to the amplifier if the apparatus is properly designed, while others put the blame directly on the diaphragm of the loud-speaking device. The sound pictures of Professors MacGregor-Morris and Mallett are very convincing in this respect, and one feels obliged to agree with Mr. Pocock when he remarks that, "Present-day electromagnetic loud-speakers are without exception a compromise between relatively good efficiency and good quality." The main line of attack towards better articulation is to obtain an improved form of diaphragm. One or two practical suggestions have been made to overcome this defect, but those responsible for them seem to admit that they are still a long way from the correct solution. In an attempt to solve this problem I have devised a

system in which the inherent inertia and the natural frequency of the diaphragm have been completely eliminated. This is accomplished by vibrating the diaphragm continuously at a slightly supersonic frequency by electrical means, and then producing the required articulation either by modulating the energising source or by varying the friction between two rubbing contacts to obtain the same result, as in the case of the Brown "Frenophone" or the Johnsen-Rahbek loud-speaker. The active part of the diaphragm of this apparatus may be as much as 8 or 10 in. in diameter. Among the advantages of this system is the fact that a very much larger volume of sound can be controlled by a single unit than has hitherto been possible.

Mr. F. E. Smith (*communicated*) : Professor Rankine has suggested that it ought to be possible to make a direct comparison between the original and the reproduced sounds. Such a comparison is possible and can easily be made by means of an oscillograph if the latter enables two records to be made simultaneously. In practice the speaker (or source of sound) and the receivers hould be housed in the same building, an easy matter to arrange in the 2LO station. In addition to the oscillograph, two telephone receivers, as identical as possible, would be needed ; these receivers should be connected to the oscillograph, and when a sound is made in front of them the two traces should be practically identical. Distance and echoes must, of course, be taken into account. In an experiment with speech the speaker would broadcast as usual, but one of the telephone receivers would be placed near the transmitting microphone, and the other would be acted on by the reproduced speech from the loud-speaker. Two oscillographic records would thus be obtained, one corresponding to the original and the other to the reproduced sound. The difference between the two records would be due to the distortion produced in the link, made up of microphone, amplifier, transmitting set, receiving set and loud-speaker. Such an experiment has not to my knowledge been made, but it presents no great difficulty. I have taken many speech records with oscillographs and obtained excellent results. Records of speech would not be very useful. A good plan would be to have fairly pure tones from tuning forks varying in frequency from 50 to 5,000 At first, single tones should be transmitted, but afterwards it would be well to transmit two notes simultaneously. Differences in amplification should not be difficult to detect by analysis of the resultant curves.

Mr. L. W. Wild (*communicated*) : I desire to discuss in some detail a cause of tone distortion which is very generally met with and which I consider has not yet received adequate attention. It is common practice to operate a loud speaker by means of two low-frequency valves. It is the distortion occasioned by the valve coupling that is in my opinion the worst offender in receiver design. This coupling is generally effected by means of an iron-cored transformer with a moderate step-up ratio. The standard pattern of transformer has a primary inductive impedance of about 10,000 ohms on 260 periods (middle C). The valve preceding the transformer has an internal impedance round about 100,000 ohms. An unloaded transformer working below its resonance frequency (about 1,500 periods) may be considered to be a simple inductance. The valve impedance may be considered to be simply a resistance. We thus have a resistance in series with an impedance, the former being the predominant partner at all frequencies below resonance. An inductance has the property that its current and E.M.F. waves are different in form, unless both are pure sine waves. Every harmonic present in the current wave is intensified

in the E.M.F. wave. For example, a third harmonic in the current wave comes out exactly three times as strong in the E.M.F. wave. It follows from the foregoing that on all fundamental frequencies except for about one octave near the resonance point, the harmonics in the current wave will be substantially the same as in the E.M.F. wave impressed upon the grid of the first valve, and, in consequence, the harmonics in the E.M.F. wave impressed upon the transformer and passed on to the grid of the second valve will be greatly intensified thus causing tone distortion. Could, however, the inductive impedance of the transformer be greatly increased so that throughout the musical scale it was several times the impedance of the preceding valve, we should have an E.M.F. wave impressed upon the grid of the second valve substantially similar to that impressed upon the grid of the first valve, with practically no tone alteration. I have worked out a design for a transformer having an even ratio and an inductive impedance of 2 megohms on 260 periods, or about 20 times the impedance of an average valve. The weight of this transformer would be 40 lb. and I am afraid that it would not be considered an ornament in the drawing-room. But why use a transformer at all? A simple inductance constructed on the same lines, but without a secondary winding, can be made having the desired impedance and not weighing more than 5 lb., which is, I think, within the limits of size, weight and cost requisite to meet with approval. I have made several of these inductances and have distributed them amongst my friends, all of whom report that by substituting these for their former transformers they have effected a marked improvement in tone reproduction besides obtaining an increase in volume of sound. The increase in volume is no doubt due to the fact that full advantage is taken of the amplification factor of the first valve, which is not the case with a transformer, and is not compensated for by the step-up ratio. These inductances have an inductive impedance of 2 megohms and a capacitative impedance of rather over 4 megohms on 260 periods. Their impedance is therefore several times that of the preceding valve over the whole range of the piano scale. The same tone reproduction can be obtained with the well-known but little used resistance coupling, but under the best conditions the amplification realized with this form of coupling cannot exceed half that obtained with inductive coupling if the same number of high-tension cells are used. Because I condemn the employment of transformers for valve coupling it does not follow that I cannot find a place for them in some part of a receiver. A potential transformer following a crystal and preceding a valve is a perfectly legitimate appliance to use, provided that no high-frequency amplification is employed. A treated galena crystal has an impedance of only about 400 ohms, and if the transformer has a low primary resistance, an impedance of about 10,000 ohms on 260 periods and not too much self-capacity, there should be practically no tone distortion. To design a potential transformer free from tone distortion for employment in this position is not difficult. What is more difficult is so to adapt the design as to obtain the maximum volume of sound, and, for this purpose, consideration has to be given to the oscillating circuit from which all power must be drawn. Most of our present transformers are fairly efficient in this respect, but could probably be improved somewhat by about trebling their weight.

Dr. W. A. Aikin (*communicated*): Without going too deeply into the science of phonology, which is a special branch of physiology for the study of the vocal organs and their functions, I think that some of its facts and principles might be useful in the discussion on loud-speakers. We learn from the natural phenomenon known as the

" resonator scale " that the resonation of vocal sound is two-fold. The upper principally in the mouth, is a complex of high reinforcements with a definite resultant, giving vowel-character ; whereas the lower, principally in the neck, expands for deeper reinforcements, giving " tone " or general quality to the sound. By this arrangement sonorous quality increases the amplitude of the sound waves by reinforcing deep harmonics, and the special characters of vowels may be represented by furrows of various design upon the vibrations. Remembering that sound waves are in three dimensions, the delicacy of the sense which detects the shapes as well as the amplitude and frequency of the vibrations as they fall upon the drum of the ear, is wonderful indeed. In declamation we rely more upon deep resonation than upon force, and therefore the mechanical loud-speaker should be constructed with a view to preserving and even augmenting this function, if the natural " humanity " of speech is not to be impaired. I would therefore draw particular attention to the deeper effects of resonation in any experiments that are made ; for the smaller vibrations of character will come through almost anything, whereas the deeper effects of tone are easily destroyed. Mentioning experiments reminds me that I sought for years in vain for a laboratory where acoustic experiments could be carried on. Then electricity and chemistry crowded out all else. Now, perhaps, someone will take up sound and make the best of mechanical voices on the same principles as the phonologist does of natural ones. Among the Papers which I have written on the subject, I think that one read at the Society of Arts in 1906 and printed in their journal would indicate to anyone the laws we follow. The subject of resonation is not yet fully investigated, and our greatest authority, Helmholtz, left behind him several unaccountable errors in his work on vowel sounds which have delayed our correct knowledge of the facts. His definition of the shape for the vowel AH, for instance, as " a funnel increasing with tolerable uniformity from the larynx to the lips," is anatomically impossible. Any loud-speaker made upon that pattern would, in my opinion, cut off much of the full quality or tone reinforced in the lower hollow and emitted through an open throat.

Mr. L. Miller (*communicated*) : Prof. Rankine alludes to thermal receivers. Like many other useful inventions, these originated in this country and have then been almost entirely dropped, while other nations have continued to develop them. Many patents have been taken out on the Continent and in America for heat-operated receivers, chiefly of the glow-lamp type, in which the air takes up the vibrations from a hot wire, no diaphragm being employed. Such receivers have been placed on the market, but it is obviously very difficult to compete with such a good and heavily-capitalized instrument as the ordinary electromagnetic telephone. I have had no experience with the thermal receivers in question, nor with the straight-wire kind with one end fixed to a diaphragm, but have made many experiments with the loose-contact receiving microphone type, which, in my opinion, are also actuated by expansion and contraction. I gave a demonstration with them in this hall before the Wireless Society in November, 1921. They reproduce speech and music very clearly, but not at all loudly, as compared with the ordinary telephone. As claims have been made in the electrical Press (though not by me) that they can be used for loud-speaking purposes, I have recently tried at what distance from one of my receivers speech could be heard so as to be understood, and I found it to be about 3 ft. No horn was employed, and one contact of marcasite was fixed in the centre of a large mica diaphragm, slightly pressed by another contact of hard synthetic

galena. It seems very unlikely that the instrument has reached anything like finality in my hands alone and I shall therefore conclude by endorsing the words of Prof. Fleming, in the last Kelvin Lecture,* that it is well worth further study.

Replies to the Discussion.

Professor A. O. Rankine: The outstanding fact which emerges from this discussion is that apparently we cannot yet get sufficient loudness for loud-speaking purposes without relying to a considerable extent upon resonance. Perhaps in the future there will be developments on the practical side which will enable us to escape from this position, and I feel sure that this would lead to great improvement. For the present, however, we have to make the best of the actual situation, and this fact makes my opinion coincide with that of Mr. Nash, who has, I think, misinterpreted my remark about horns. I would advocate the elimination of these devices only if up to the point where their function begins the acoustic output has already suffered no distortion from resonance. If, on the contrary, as Captain Cohen has so clearly explained, present circumstances compel us to choose between pure quality and enough intensity, I agree with Mr. Nash as to the value of horns in that they may render resultant resonance less selective. They may, as I have already indicated in my introductory remarks, be used to provide additional resonances so as to balance other inevitable ones in other parts of the essential frequency scale. It would, however, be a rather fatal mistake to fashion them to suit any special vowel, as is half suggested at the end of Dr. Aikin's remarks. Mr. Smith has suggested an arrangement suitable for visual observations on the quality of transmission. In this system the original sounds on the one hand, and the loud-speaker output on the other, are to be picked up respectively by two "as nearly as possible identical" telephone receivers, and the results recorded simultaneously on an oscillograph. I should prefer to impose the additional condition that the telephone receivers should be completely non-resonant; and I should not feel happy about the oscillograph itself unless it possessed the same negative attribute. It is a little surprising that Mr. Smith did not specify a cathode-ray oscillograph. Perhaps he recognises no other! Mr. Sutherland in his introductory Paper speaks of "desirable reverberation," more particularly in relation to music, but also as regards speech; but I still adhere to the opinion that reverberation is a thing which we have to put up with in order to hear loudly enough, and that we have virtually to learn several different languages, musical and otherwise, according to the conditions of listening. Does a speaker or a band, after all, sound so "dead" in the open air if we are close enough to hear? I disagree, however, rather reluctantly, for Mr. Sutherland's view seems to provide me with an excuse for inability to appreciate some modern music. If a correct amount of overlapping and blending by the agency of reverberation forms an essential part of the music, it might with some force be argued that I had not yet been fortunate enough to listen to the work of, let us say, Mr. Arnold Bax, in the right concert hall.

Mr. L. C. Pocock: Mr. Marris disagrees with the statement that a horn is the ideal form of coupling between the diaphragm and the air, and considers that one becomes tired of the horn resonance; further, he states that the presence of horn resonance can be proved by removing the horn. I should like to point out, however, that

* Journal I.E.E., Vol. 61, p. 613 (1923).

the use of a horn cannot be so lightly dismissed. It is easy to design a horn with strong resonance, and this is done in horn-type musical instruments; it is also possible, though less easy, to design a horn with much weaker resonance. The former is analogous to a resonance transformer or resonant transmission line, the latter to a transmission line in which the inductance and capacity vary continuously from end to end, and such an arrangement may, in principle, be a maximum energy coupling, although the application of the principle is restricted by the length of the horn. One may easily tire of the resonance of a bad horn, but a good horn has a far from evident fundamental, with many partials, and it can scarcely be claimed that the change of tone on removing a horn of this type proves anything, because the increased diaphragm resonance resulting from reduced damping introduces far more distortion than was caused by the horn.

Mr. Voigt endeavours to show that the efficiency of a loud-speaker is considerably greater than 1 per cent. In a reacting circuit of the kind which he describes, the oscillation point is conditioned by $\eta_1\eta_2\eta_3=1$, where η_1, η_2, are the loud-speaker efficiencies and η_3 is the amplifier power-efficiency. Mr. Voigt overlooks the fact that the power amplification is the square of the voltage amplification, so that if $\eta_1=\eta_2$, and $\eta_3=(25)^2$—using the lowest figure assumed—the loud-speaker efficiency at resonance is only 4 per cent., and the average efficiency a good deal lower. It is rather doubtful whether types of receivers having less-pronounced resonance peaks would sustain oscillations with so small an amount of amplification as that described. My own calculation of the efficiency as less than 1 per cent. was based upon the estimate that normal speech represents an average emission of energy at the rate of 125 ergs per second (Physical Review, March, 1922) and that the most efficient loud-speaker obtainable requires approximately 0·0005 watt to produce the same speech intensity. The efficiency is, therefore, about 2·5 per cent., and many loud-speakers have only one-tenth of the efficiency of the one quoted. The estimated input power of 0·0005 watt was calculated from experimental data on the R.M.S. current giving the required sound intensity, and on the impedance of the receiver at 800 periods per second.

Mr. H. L. Porter: One conclusion which may be drawn from this discussion is that electrical and acoustical resonance are mainly responsible for distortion in the loud-speaker. The introductory Papers, however, were confined to a specialised consideration of one or other of the phenomena, but little attention has been paid to the need for their correlation. The discussion itself did not make this clear. For example, to manipulate the electrical circuit independently of the acoustic system could easily do more harm than good, for one might actually eliminate from the circuit resonances which might be permitted to play a very useful part in a suitably-chosen acoustic system. In our work on the gramophone, we found that the resonance of parts of the system which we anticipated might greatly add to the distortion proved to be very useful in arriving at our curve of uniform distribution of intensity. Again, we found that the greatest distortion did not arise from the separate resonance peaks but from their close acoustical proximity; the orderly arrangement of these peaks was most essential. It therefore seems reasonable to regard the problem of the loud-speaker in a similar manner. If an electrical circuit be used in conjunction with an acoustical system, the combination should be treated as a unit. The contribution of each part of that unit should be ascertained and, if possible, arranged for mutual assistance. In the past, work on the loud-speaker has been

mostly concerned with the efficiency of the electrical circuit. There may now be the danger of investigations taking an acoustical bias. Since the final sound waves are determined both by the electrical circuit and by the acoustic sytem, further work should not be allowed to take what might easily be divergent paths. Our minds must be alert to both considerations.

Professor E. MALLETT: In the original telephone receiver and in some loud-speakers the diaphragm performs a double function: it not only provides the necessary vibrating surface which moves the air particles about and so starts a sound wave, but also itself forms the mechanical system which is vibrated by the alternating speech current. In these cases resonance in the diaphragm is doubly important. In other instruments the vibrating system is a reed or an armature with spring control, each of which will have its own peculiar resonance acting to modify the amplitude of the diaphragm at the point where it is fixed to the vibrating system, but leaving the resonances of the diaphragm still to control the distribution of amplitude over the surface of the diaphragm, and hence the emission of sound waves. In reply to Mr. Burnand, it is thought that these diaphragm resonances will occur whatever the form of the diaphragm, including the cases which he cites.

Many of the speakers have confined their attention to the resonances of the horn, and the various curves exhibited are ascribed to horn resonances. But they are in all cases to be ascribed rather to the combined mechanical resonance of reed (or other vibrator which may be a diaphragm), diaphragm and horn. One speaker, Mr. Nash, shows a curve purporting to separate these out, but the curve is admittedly a theoretical one, and none of the theory is given. But if Mr. Nash's figure for the permissible variation of power output with frequency for no appreciable distortion is correct, i.e., a variation from 100 to 850, or 750 per cent., one might be inclined to ask, "Why all this fuss about resonance?" The answer seems to me to be this, that resonance may accentuate unduly a tone which was not originally transmitted. Consider, for instance, an ordinary receiver through the coils of which is flowing a current due to two simultaneously maintained notes at the transmitting end of frequencies $\omega_1/(2\pi)$ and $\omega_2/(2\pi)$. Now let the current be $i_1 \sin \omega_1 t + i_2 \sin \omega_2 t$. The pull on the diaphragm will be proportional to $(B_0 + b_1 \sin \omega_1 t + b_2 \sin \omega_2 t)^2$, where B_0 is the permanent flux, and b_1 and b_2 are the fluxes due to the currents i_1 and i_2. This expression

$$= B_0^2 + \tfrac{1}{2}b_1^2 + \tfrac{1}{2}b_2^2 + 2B_0 b_1 \sin \omega_1 t + 2B_0 b_2 \sin \omega_2 t + b_1 b_2 \cos (\omega_1 - \omega_2)t - b_1 b_2 \cos (\omega_1 + \omega_2)t - \tfrac{1}{2}b_1^2 \cos 2\omega_1 t - \tfrac{1}{2}b_2^2 \cos 2\omega_2 t$$

Thus it is seen that alternating pulls of frequencies $2\omega_1/(2\pi)$, $2\omega_2/(2\pi)$, $(\omega_1 - \omega_2)/(2\pi)$ and $(\omega_1 + \omega_2)/(2\pi)$ are introduced as well as pulls of frequencies $\omega_1/(2\pi)$ and $\omega_2/(2\pi)$ of the original tones. It is true that the amplitudes of the additional pulls are small compared with those of the transmitted tones, provided B_0 is large compared with b_1 and b_2, but they will be appreciable if b_1 and b_2 are increased by overloading the receiver, and the amplitude of the resulting sound wave may be appreciable with quite ordinary loads if the frequency happens to be near a resonance point. With the harmonic tones $2\omega_1/(2\pi)$ and $2\omega_2/(2\pi)$ the resulting effect will probably not be unpleasant, but the summation and difference tones, $(\omega_1 + \omega_2)/(2\pi)$ and $(\omega_1 - \omega_2)/(2\pi)$, may well produce most unpleasant results. This will apply to all those loud-speakers in which there is a permanent magnetic pull on the vibrating system.

Similar considerations apply to the valve if the latter is overloaded, so that either the curved part of the characteristic is used or grid current is allowed to flow. The anode current from a double-frequency E.M.F. on the grid will then contain summation and difference terms, which if they happen to be those of resonance frequencies of the receiver may produce appreciable sound waves. It may also be that with very large sound amplitudes this effect may also appear in the throat of the horn. It would seem possible, therefore, that the question of distortion is more a question of summation and difference tones than of the relative amplitudes of the transmitted tones. The above considerations would explain satisfactorily why telephone receivers and a loud-speaker worked to give a small sound output give really good reproduction, but that directly the output is increased distortion appears, and the greater the sound output the greater the distortion.

Captain P. P. ECKERSLEY: Mr. Nash has raised the question of the horn. Briefly, the horn gives efficiency, whilst the large-diaphragm, hornless type is inefficient. If, however, the hornless type (for which I hold no brief whatsoever) gives sufficient volume with more faithful quality for an average drawing-room, it has at least served usa eful purpose. The particular quality that pleased me in the type of loud-speaker which I demonstrated, was the warmth of bass tones, coupled with an extraordinary faithfulness even in the upper register. This quality might be manifest in a hornless or horn type, the low resonance being in no way intrinsic to the absence of a horn. I still reiterate that what I have called the "efficient" type of loud-speaker with resonances around 1,000 periods per second can never give faithful reproduction, and that lack of warmth in the bass, a minimisation of higher harmonics, and a barking of vowel sounds in speech are all characteristic of this type of instrument. Our ears are most sensitive to this middle register, and are asking all the time for stronger extremes of the sound gamut. This must be the next great advance in loud-speaker design; whether the horn will be used or not is immaterial to the question of proper frequency recognition.

PUBLICATIONS OF THE PHYSICAL SOCIETY.

THE SCIENTIFIC PAPERS
OF THE LATE
SIR CHARLES WHEATSTONE, F.R.S.
Demy 8vo, cloth. Price 12s.; to Fellows, 6s.

Uniform with the above.

THE SCIENTIFIC PAPERS OF
JAMES PRESCOTT JOULE, D.C.L., F.R.S.
Vol. I. 4 Plates and Portrait, price 18s.; to Fellows, 9s.
Vol. II. 3 Plates, price 12s.; to Fellows, 6s.

PHYSICAL MEMOIRS.

PART I.—VON HELMHOLTZ, On the Chemical Relations of Electrical Currents. Pp. 110. *Price 6s.; to Fellows, 3s.*

PART II.—HITTORF, On the Conduction of Electricity in Gases; PULUJ, Radiant Electrode Matter. Pp. 222. *Price 12s.; Fellows, 6s.*

PART III.—VAN DER WAALS, On the Continuity of the Liquid and Gaseous States of Matter, Pp. 164. *Price 12s.; to Fellows, 6s.*

REPORT ON RADIATION AND THE QUANTUM THEORY.
By J. H. JEANS, D.Sc., LL.D., F.R.S.
Second Edition Now Ready.—*Price 7s. 6d.; to Fellows, 3s. 9d. Bound in Cloth, 10s. 6d.*

REPORT ON THE RELATIVITY THEORY OF GRAVITATION.
By A. S. EDDINGTON, M.A., M.Sc., F.R.S.
Plumian Professor of Astronomy and Experimental Philosophy, Cambridge.
Third Edition.—*Price 6s.; to Fellows, 3s. Bound in cloth, 8s. 6d.; to Fellows, 6s.*

REPORT ON SERIES IN LINE SPECTRA.
By A. FOWLER, F.R.S.,
Professor of Astrophysics, Imperial College of Science, South Kensington, London.
Price to Non-Fellows, 12s. 6d.; to Fellows, 6s. 3d. Bound in Cloth, 15s., to Fellows, 9s.

THE TEACHING OF PHYSICS IN SCHOOLS.
Price to Non-Fellows, 1s. 6d. net, post free 1s. 8d.

METROLOGY IN THE INDUSTRIES.
Price to Non-Fellows, 1s. 6d. net, post free 1s. 8d.

DISCUSSION ON LUBRICATION.
Price to Non-Fellows, 1s. 6d. net, post free 1s. 8d.

DISCUSSION ON ELECTRIC RESISTANCE. *Price 2s. 6d. net.*

DISCUSSION ON HYGROMETRY. *Price 5s. net.*

PROCEEDINGS.

The "Proceedings" of the Physical Society can be obtained at the following prices:—

Vol. I. (3 parts) bound cloth, 22s. 6d.
Vols. II., IV., V., XXIII., XXV., XXVI., XXVII., XXVIII., XXIX., XXX. & XXXI. (5 parts each), cloth, 34s. 6d.
Vols. III., VI. to XII. & XXII. (4 parts each), bound cloth, 28s. 6d.
Vol. XIII. (13 parts, each containing Abstracts), bound cloth (without Abstracts), 70s. 6d.
Vols. XIV. & XV. (12 parts, each containing Abstracts), bound cloth (without abstracts). 34s. 6d.
Vols. XVI. & XIX. (8 parts each), bound cloth, 52s. 6d.
Vols. XVII., XVIII. & XXI. (7 parts each), bound cloth, 46s. 6d.
Vols. XX. & XXIV. (6 parts), bound cloth, 40s. 6d.

Most of the parts can be purchased separately, price 6s., by post 6s. 3d. Fellows can obtain the *Proceedings* (in parts) for their personal use at half the above prices.

ABSTRACTS OF PHYSICAL PAPERS FROM FOREIGN SOURCES.
VOLS. I. (1895), II. (1896), III. (1897), 22s. 6d. each; Fellows, 11s. 3d.

Strong cloth cases for binding the "Proceedings," price 3s. 6d. each, post free.

BLAKESLEY, T. H. A Table of Hyperbolic Sines and Cosines. *Price 2s. 3d.; to Fellows, 1s. 2d.*

LEHFELDT, R. A. A List of Chief Memoirs on the Physics of Matter. *Price 3s.; to Fellows, 1s. 6d.*

Applications for the above Publications should be sent direct to
FLEETWAY PRESS, LTD.,
3-9, DANE STREET, HIGH HOLBORN, LONDON, W.C.1

CONTENTS.

PAGE

VII. Æolian Tones. By E. G. Richardson, M.Sc. 153

VIII. The Effects of Torsion on the Thermal and Electrical Conductivities of Metals. By J. E. Calthrop, B.A., B.Sc. 168

IX. A Generalisation of Professor Whitehead's Theory of Relativity. By G. Temple, B.Sc. 176

X. On the Structure of the Atomic Nucleus and the Mechanism of its Disintegration. By Dr. Hans Pettersson 194

XI. The Effect of a Magnetic Field on the Surface Tension of a Liquid of High Susceptibility. By Winifred L. Rolton, B.Sc., and R. Stanley Troop, B.Sc. 205

Demonstration of an Auxiliary Optical System for the Examination of Crystal Sections by Conoscopic Methods. By C. W. Hawksley 210

Discussion on Loud-Speakers for Wireless and Other Purposes (*Concluded*) 211